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THE REACTION BREAKWATER AS PROPOSED FOR THE OPENING OF THE SOUTH-WEST PASS OF THE MISSISSIPPI RIVER.

BY LEWIS M. HAUPT, Member of the Institute.

INTRODUCTORY.

The Louisiana purchase of 1803 had for one of its objects the control of the outlet of the great central basin containing over 15,000 miles of navigable waterways, at a time when railroads were unknown.

Since 1837 the Government has made numerous desultory efforts to increase the depths at the delta mouths, which ranged from 8 to 15 feet, but without permanent results.

On May 3, 1875, after a protracted debate, Congress determined to depart from its usual policy and awarded a contract for the deepening of the South Pass to Capt. Jas. B. Eads for $5,250,000, on the condition of making payments

only as results were secured. After overcoming many difficulties in the face of persistent opposition, he secured a channel of 26 feet navigable depth and 700 feet surface width, which has been maintained to the present time, but this contract having now expired by limitation, and the dimensions of vessels having outgrown the channel, the problem of providing additional facilities is now demanding immediate and careful investigation.

On January 7, 1899, a Board of Engineers reported that "The following project, if executed with vigor, will, in the opinion of the Board, secure a navigable channel of adequate width and of 35 feet depth at mean low water of the Gulf of Mexico, throughout Southwest Pass of the Mississippi River, at a cost which is not excessive considering the vast commercial interests involved, and therefore in the opinion of the Board the proposed improvement is practicable."

Briefly, the plan consisted of two parallel straight jetties placed 2,400 feet apart, to extend from "about 30 feet of water" on the outer slope of the bar for about seven miles up the pass, making over fourteen miles of jetties in all, which with the levees, sills and auxiliary works were estimated to cost $13,000,000, while the estimate for jetty extension and maintenance was $390,000 annually, equivalent to another $13,000,000 at 3 per cent. In closing this report the Board remarks that "The plans described, therefore, are such as have been selected for the purpose of estimate, and should not necessarily be strictly adhered to unless experience shall fully justify them." "It is desirable that at least two powerful dredges be constructed at the beginning of the work, for use in forming and maintaining the required channel."

These plans, which were, therefore, regarded as merely tentative and designed mainly as a basis for an estimate, were referred back by Congress to a board of four engineers, "of whom at least two shall be from civil life; * * * said board of engineers shall submit detailed estimates of the cost of each and every feature of the project, and they shall report especially whether it is necessary to construct inner

jetties; and if, in their judgment, inner jetties should be constructed, they shall provide for the location of the same, so as to involve the least cost consistent with the safety and efficiency of the work hereby contemplated."*

This board reported, January 11, 1900, a radically different plan, "placing the main reliance upon dredging," and depending upon a "bottle or coffin"-shaped plan of jetties to aid in maintaining the channel. These jetties are to be divergent from the ends of the fast land, to be placed in shallow water remote from the channel, and to extend only to the outer 20-foot curve, all "for ease and cheapness of construction." "Their distance apart between centers varies from about 7,000 feet to a minimum of 3,000 feet. This width, even where it is least, is greater than has been heretofore deemed advisable * * * to take from them the duty of forming the channel."

In short, it would appear that the jetties will act to impound the silt in the vicinity of the channel instead of operating as a barrier to prevent its return after being pumped over, and are of no other use. Moreover, it seems that no consideration was given in this report to the quantity of silt the dredges would be obliged to handle, although it is stated that to create the desired channel would require the removal of some 22,000,000 cubic yards from the bar, and that two dredges could do this work in two or three years at a cost not exceeding 4 cents per cubic yard.† It was stated that the purpose of the modifications made in this second report was mainly to reduce the cost. The report, therefore, closes with the same recommendations as its predecessor, reserving the letting of the superstructure "until experiment and experience have developed the most economical methods of construction."

It would, therefore, seem that the problem is one that demands early consideration, and a solution which will give reasonable assurance of results within a cost which shall be fully justified.

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* River and Harbor Act, March 3, 1899.

† *Vide* p. 4, 56 Cong., 1st Sess., H. R. Doc, 329.

THE PHYSICAL PROBLEM.*

The possibility of securing and maintaining a deep-water channel at this pass can best be answered by a study of the

Proposed Government plan with two jetties and dredging.

Proposed reaction jetty without dredging for maintenance.

work done by the stream itself under its normal conditions, regarding it as an endless belt charged with sediment.

*Based upon the data furnished by the survey of J. A. Ockerson, C.E., member Mississippi River Commission, 1898.

If the favorable conditions existing in the pass can be reproduced on the bar in such manner as to create erosion instead of sedimentation and cause the deposition of the silt in a zone remote from the channel, the problem is solved in the best manner by utilizing natural agencies to carry its silt out of the pathway of navigation.

The usual method of improving the mouths of sedimentary rivers consists of the construction of two parallel jetties, at suitable distances apart, to confine the effluent until it reaches deep water on the outer slope of the bar. Such a method is effective, but it has the serious objection of recreating conditions at its mouth similar to those existing in a state of nature. The two jetties, acting as an aqueduct, must transport all the river sediment through the canalized channel and dump it in the sea at its mouth, trusting to the external forces to distribute it. This requires constant attention and dredging unless there be a strong littoral movement. Straight parallel jetties do not concentrate currents.

The modification proposed by the writer consists of a single curved jetty so located as to produce a reaction and scour across the entire bar in such a manner as to effect a lateral displacement of the material and the deposition of this silt on the opposite bank of the channel, where it will form a natural levee automatically adjusted to the regimen of the stream.

The probability of the success of this system can best be seen after a consideration of the physical condition of the stream and a careful calibration of the bar and channel.

PHYSICAL—THE BAR.

The form of the crest of the bar is an elongated ellipse having a semi-transverse axis of 3·5 miles and a conjugate axis of 1·9 miles. (See cut, Pl. 2.)

Its total perimeter from East Point around to the opposite bank is 7 miles.

The width of the bar between the 35-foot contours in the river and gulf is slightly over 3 miles and the ruling minimum depth of water is about 10 feet, extending over a length of nearly 2 miles measured along the crest.

PROFILE AND SLOPES OF BAR.

From a depth exceeding 40 feet, the bed rises 14 feet in less than 1 mile, or 1 foot on 360, thence it flattens in the next 2 miles to 1 on 690, running nearly level for several thousand feet and suddenly dropping off with an outer slope of about 1 on 12 to the 35-foot contour. Thence it deepens rapidly to over 70 feet within a half mile.

This profile is characteristic of a delta bar rolled seaward by a sedimentary river. All the material passing over it must first be dragged up the inner slope by friction or be carried in suspension by the ebb stream. With the bar removed the resistance to the transportation of sediment would be greatly reduced, and if a lateral instead of a longitudinal movement be imparted to it, the path will also be shortened and the work required of the current be still further diminished. In short, the bar would be rolled over laterally instead of being pushed out into the gulf in front of the jetties.

The longitudinal area of the profile of the bar above 35 feet depth, as it existed in 1898, was 265,375 square feet.

The length at bottom was about 16,700 feet, so that the mean depth of the deposit above 35 feet was 16 feet.

HYDROGRAPHY OF THE STREAM.

From the head of the pass to the outer 35-foot contour the distance is 19½ miles,of which about 15 miles are enclosed between two nearly parallel but low banks built by deposits from the stream. Beyond this point the process of bank-building is constantly progressing, but it is moulded and resisted by the exterior forces of tides, waves, winds and currents.

It should be noted, as the measure of the depths that the energy of the stream can maintain throughout the first 12 miles, that there is nowhere less than 45 feet along the thalweg of the channel, with occasional depths of 98 and 100 feet, while the mean cross-section between the first and eleventh mile is about 66,000 square feet and the average width is 1,511 feet, giving a mean depth of approximately 44 feet.

The quantity of discharge varies greatly at different seasons of the year, being greater in the spring; but taking the average at 200,000 cubic feet per second flowing through the mean cross-section, there will result a velocity of 3 feet per second (2 miles per hour), which is sufficient to move small stones of nearly 2 inches in diameter. It will not, therefore, permit deposits of sand or mud and is amply sufficient to maintain the sectional area.

The observed velocities varied from about 4·9 feet in May to 1 foot in August, when the discharge was least. It is also found that 41 per cent. of the entire discharge traverses the Southwest Pass; 9 per cent. the South Pass and the balance, or 50 per cent., the Pass á Loutre. But this 41 per cent., on reaching the gulf, where it is unconfined by restraining banks, expands externally and escapes over the bar through a section of 175,125 square feet, resulting in a mean velocity of a little more than 1 foot per second, with much lower bottom velocities. The mean depth, if uniformly distributed, would be 8·1 feet, but in consequence of the concentration due to inertia the actual depths at the apex of the bar are about 10 feet and on the flanks from zero to 3 feet. The width here is about seven times the normal width of the stream and no plan of improvement can be expected to permanently maintain the depths which does not provide for the approximate restoration and maintenance of this normal width and section.

If the width is materially enlarged it must be at the expense of depth, or the velocity will be greatly reduced and, in consequence, the rate of shoaling will be largely augmented by the effort of the stream to repair the violence done to its regimen.

THE ALIGNMENT.

But there are other agencies at work in the stream than mere volume and velocity which tend to create and maintain depths, as may be seen by a critical study of the location of the line of greatest depths with reference to the alignment of the axis of the river's bed, which have not received the attention their importance merits.

Thus, starting at the head of the pass, the river is practically straight for over 3 miles, with maximum depths diminishing, notwithstanding the contracting banks, from 76 to 58 feet. It then enters a curve to the right having a radius of 4·4 miles, which it follows for 2 miles, with depths increasing to 73 feet. Then comes a short straight reach of 4,000 feet, with depth decreasing to 69 feet, followed by a sharper curve to the left (nearly 3-mile radius), resulting in 91 feet depth at end of the seventh mile and a reverse curve to the right, of 5¼ miles radius, reaching over nearly 2 miles with 75 feet depths.

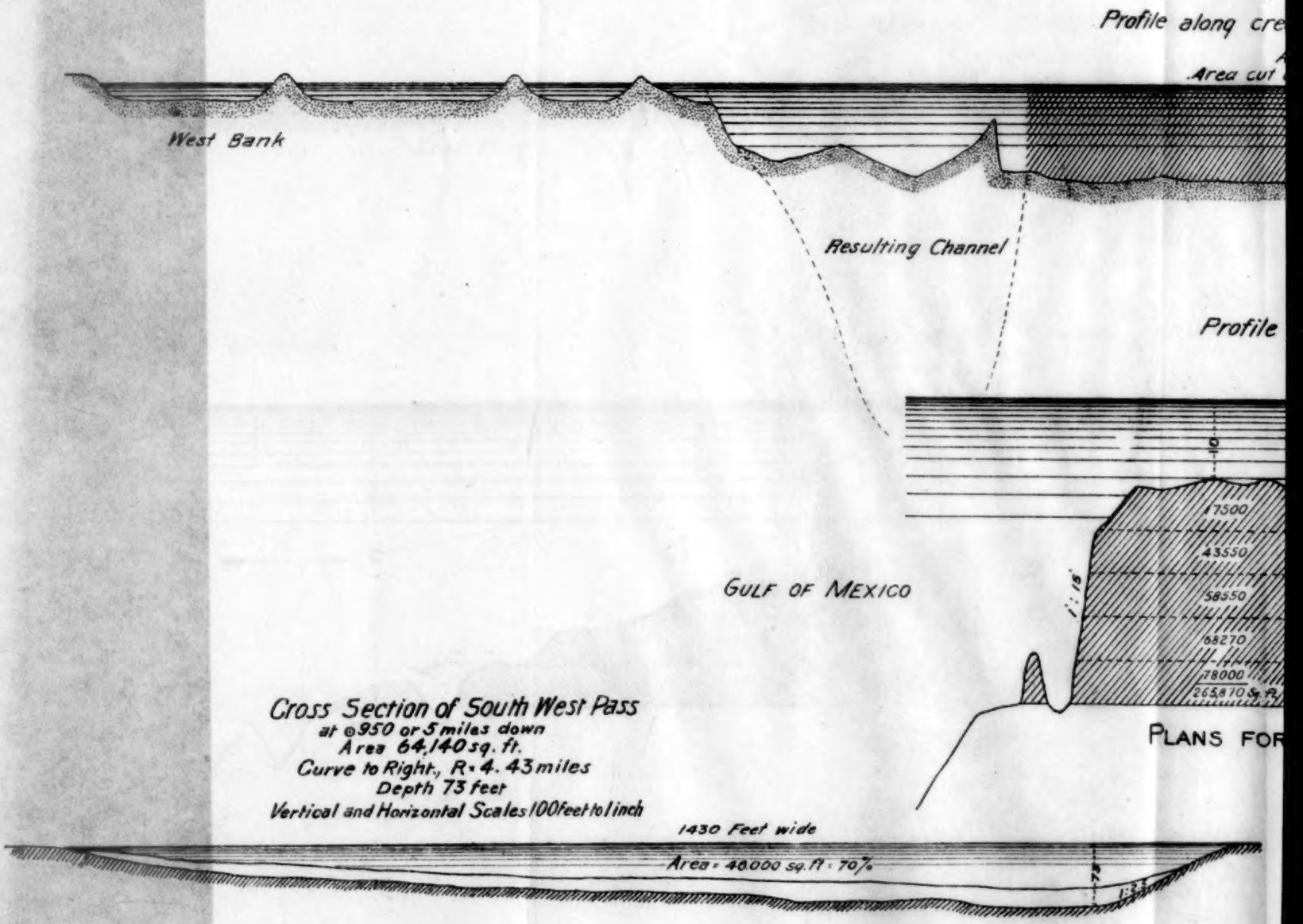
From this ninth mile the course is nearly straight to the bar, with widening banks and diminishing depths as follows: at the beginning of the tenth mile, 68 feet; eleventh, 57; twelfth, 52; thirteenth, 43; fourteenth, 36; fifteenth, 32; sixteenth, 38; seventeenth, 31; eighteenth, 21; at the end of the eighteenth mile, 15; eighteen and three-fourths crest of bar, 10; beginning of nineteenth, 15; of twentieth, 35; and of twenty-first, 93 feet.

Showing very conclusively the beneficial effects of curvature upon depth and maintenance of a channel of ample dimensions for navigation. The deepest water is found to be confined to the concavities in every instance, whilst even the crossings at points of inflection are not materially shoaled.

Hence it follows as a natural sequence that if, instead of permitting the effluent stream to discharge itself through a cross-section of 175,125 square feet, it be confined to one of about 70,000 or less, while at the same time the collateral bank is left open, as a weir, for surplus discharge, and if the reactionary forces be developed by a curve of normal radius, the results should be a reproduction of those existing in the stream above the bar and at a cost much less than that required to erect two parallel or other jetties.

Some idea of the enormous force which would be utilized by such a jetty may be ascertained from the following figures:

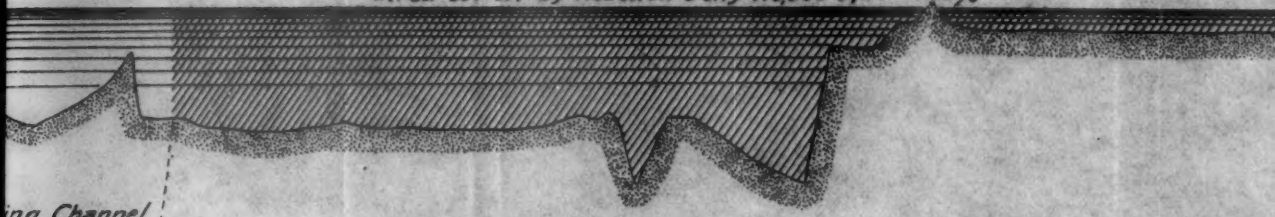
The main discharge is taken, as a basis of computation, at 200,000 cubic feet per second. The area of discharge



Cross Section of South West Pass
at ⊙950 or 5 miles down
Area 64,140 sq. ft.
Curve to Right, R = 4.43 miles
Depth 73 feet
Vertical and Horizontal Scales 100 feet to 1 inch

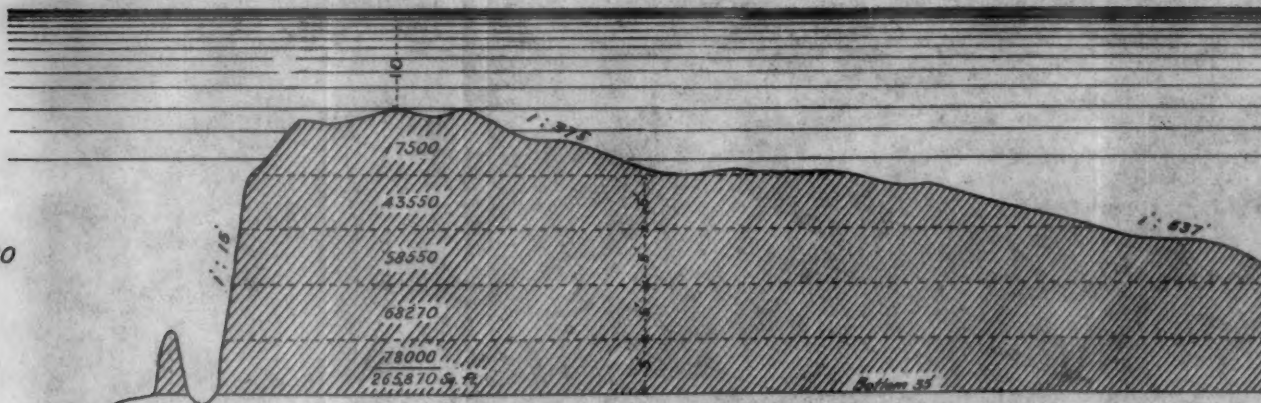
Profile along crest of Bar from East Point to Inner Lump

Area of Water section 175,125 Sq.Ft.
Area cut off by Reaction Jetty 115,000 Sq.Ft = 65%



ing Channel

Profile of Bar along existing channel



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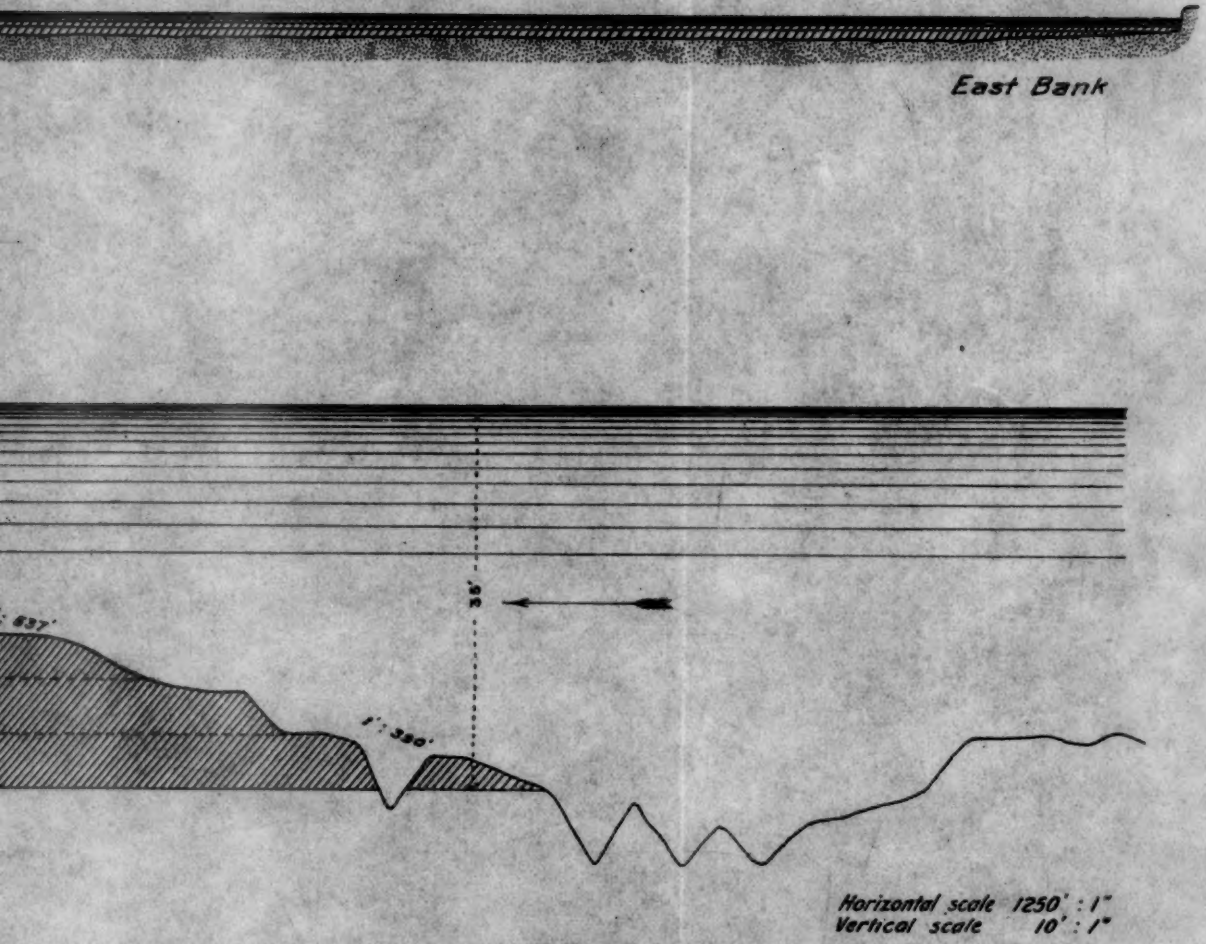
PLANS FOR THE IMPROVEMENT OF S.W. PASS MISSISSIPPI RIVER

AS PROPOSED BY

Lewis M. Haupt

(Haupt)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Horizontal scale 1250' : 1"
Vertical scale 10' : 1"

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along the bar crest is 175,000 square feet. Of this, the single jetty would cut out 115,000 square feet or 66 per cent. (See cut.) If, therefore, this percentage of the discharge be deflected from its natural egress and be confined to the immediate vicinity of the guiding jetty, it would represent 133,000 cubic feet of water, weighing 3,700 gross tons, sweeping along the face of the work at the rate of 1 foot every second, in addition to the 66,000 cubic feet, or 1,850 tons, already traversing this same area.

This result would only be true, theoretically, if the velocities through every unit of the cross-section were uniform, which is not the case in practice; yet it is approximately correct, and serves to illustrate the great increment in concentration and discharge over a limited path which must result from this form of construction.

In other words, if, instead of only one-third, the entire discharge be voided over a limited sector of the bar, there must result a threefold increase in velocity or a triple increase in the water-section across that region of the bar.

The first result would be an increase of velocity, and the second the gradual enlargement of the section as its consequence; but these results would follow the development of the work as it proceeded, and hence there would be a rapid and simultaneous improvement depending upon the rapidity of construction, for as the resultant velocity increases by contraction due to the jetty extension, the section will be enlarged by scour due to this increase and localization of currents, thus tending to restore the velocity to its normal condition, and these agencies will operate simultaneously to create and maintain a normal cross-section and velocity after the completion of the work. The more quickly it is completed, however, the more economical and rapid will be the formation of the new channel.

As previously stated, every river possesses within itself both constructive and destructive forces; constructive where its energy is dissipated, destructive where it is concentrated.

It is for the engineer to use the one and reject the other according as he desires erosion or deposit, channel or bar.

Concentration of energy is caused by compression of currents; dissipation, by expansion.

Two parallel jetties do not compress, but merely train and direct the given quantity of water entering at their head.

A single concave curved jetty, so placed as to encroach gradually upon the path of a stream, however, does produce a compression causing reaction, deflection and deep erosion creating a channel parallel to its axis and building a natural levee by the lateral transportation of the displaced material.

If properly adjusted to a sedimentary delta bar, it converts dispersion into concentration, confining the energy of the outflow to the weakest section of the bar and produces a change of equilibrium in favor of the effluent stream. In short, it flanks the bar and opens the port.

Thus, this available energy of the stream, instead of being ignored, may be trained and applied to the carving out and maintenance of its own ample waterway for the benefit of commerce and industry.

INFLUENCE OF CURVATURE UPON DEPTH.

Every pilot and navigator understands that the deep-water channel hugs the concave bank, and every physical hydrographer has observed the same fact, whilst the engineering profession has been slow to recognize its cause or to make any practical application of this potent agent in the removal of shoals or the creation of channels.

Having in view their application, a few physical facts may be stated as fundamental principles:

(1) Rivers ignore straight lines and abrupt or angular changes of direction.

(2) The bed of the channel does not mould the stream, which is the living entity, so much as the stream moulds the channel, though they are closely related. The stream is the force, the bed the resistance as well as the resultant of that force.

(3) Bars are not found in the concavities of the bends, hence if a stream is made to bend over a bar the latter must disappear.

(4) Neither velocity nor volume constitute the most important factors in erosion, but the impact due to concentrating currents, continuously supported, or, in other words, a constantly maintained change of direction, causing reaction accompanied by lateral and vertical movements.

These agencies and effects have been carefully observed and studied of late years by some of the most distinguished and successful hydraulicians of Europe. A few of the conclusions reached by these authorities will be found in the appendix, as confirming these principles.

THE PROPOSED PLAN.

The jetty which will best fulfil the requirements of this problem is the one starting from East Point and extending nearly 2 miles in almost a straight line parallel with the axis of flow and at a proper distance from it, thence curving to the westward with a radius of about 4·64 miles for a little more than 2 miles and ending at the 30-foot outer contour.

The portion of the area of discharge which would be cut out by this jetty is 115,750 square feet, or 66 per cent., which deducted from the total area would leave 59,375 square feet or less than the normal section, but as the work would require several years to build and the volume of the movement would be confined to the outflow following the line of the jetty, the formation of the channel would be local, permanent and rapid. The 15,000 square feet of water section on the west flats would ultimately partially fill up and thus assist in confining the currents to the channel.

It is not the purpose of this paper to consider the details nor cost of construction.

BAR ADVANCE.

The comparative surveys since 1838 and 1898 show that during these sixty years the outer "35-foot" contour has moved gulfward about 3 miles, giving an average rate of 259 feet per annum, although the rate during the last quarter century was but ¾ of a mile or 176 feet per annum. The building of parallel jetties confining the silt to the outer slope

of the bar would undoubtedly greatly increase this advance; the bottle-shaped plan would create two bars, one outer and one inner, whilst the single curved jetty which would deflect material laterally over the ample shoals to the westward would retard it; but assuming that it were to advance at the rate of 100 feet per annum, it would require over fifty years to travel 1 mile, and the plan of the jetty is such that it would reach 7·4 miles before covering a quadrant, while, if found necessary, a tangent or reversal of curvature can be introduced in the distant future, as in the pass above, if thought desirable by remote generations.

DREDGING.

In his interesting review of the history of operations at the mouth of the Mississippi, Major Jas. B. Quinn, U. S. E., states that the first efforts consisted in stirring up the bottom with harrows, which were moderately successful in securing a slight increase. Then jetties were combined with the stirring-up process. This failed. The necessities for commerce grew, and yet with powerful dredging machines the maintenance of a channel with as little as 20 feet depth was ineffective. Surveys were made, boards reported, theories were fiercely debated, but they all ended in recommending larger appropriations.

Then came Captain Eads, and threw himself into the breach. He was most bitterly opposed by those who should have given him greatest encouragement. He risked his life and fortune, and finally opened a channel 30 feet in depth, disproving the ominous predictions of disaster and rapid bar advance by his success. "His perseverance under such adverse circumstances (says Major Quinn) was truly remarkable."

The first effort to open the Southwest Pass was made in 1837, by using an ordinary bucket drag. No improvement resulted, as a single storm obliterated all traces of the channel.

In 1852 $75,000 were appropriated to be expended by a Board of Army and Navy Engineers, who recommended the stirring-up process, and a contract was made for a channel

18 feet deep and 300 feet wide. This channel was maintained by harrowing and dredging for a whole year.

No further appropriations being made until 1856, the 18-foot channel vanished. Then $330,000 were made available. The contractors secured and maintained an 18-foot channel only so long as operations continued.

In 1867 another contract for stirring up was made for $75,000, but not executed. Then the "Government built a powerful dredge boat of special design, which was operated under the very best conditions," but it did not succeed in maintaining a much deeper channel than that formerly secured.

"Everybody was tired of the uncertain results secured by dredging on the bars," and the Fort St. Philip Ship Canal was proposed and estimated to cost $10,273,000. The only officer opposing it was Col. J. G. Barnard, U. S. E., who advocated an open-mouthed river.

Mr. Eads proposed to improve the Southwest Pass by jetties for $10,000,000. Ultimate depth, 28 feet. This led to a fierce controversy, resulting in Eads being required to accept the South Pass at a cost of $5,250,000.

The report of the Board, submitted January 13, 1875, recommended parallel dikes, with a view of obtaining a navigable channel of 25 feet, at an estimated cost for the South Pass of $7,942,110 and for Southwest Pass of $16,053,-124.

The Board therefore recommended the South Pass as being entirely adequate and much cheaper. It stated:

"Its depth is 7 feet, and it discharges at its mouth about 22,000,000 cubic yards of sediment in suspension per annum," and added,

"These jetties should be 900 feet apart and run in straight lines parallel to each other to the 30 feet depth outside."

Fortunately, they were not built straight, but on a curve, gradually compounding to the westward, and ultimately still further contracted by spur dikes to reduce the width to about 700 feet.

The east jetty as built was 2⅛ miles long, and had a

radius at the outer end of a little over 2 miles (11,720 feet). The west jetty was about 1,000 feet distant, and had a radius of 15,000 feet.

The above digression will illustrate the difference between what was recommended and what was done, and serves to fill out the history.

Returning to the sediment and its removal by dredging, there is considerable range of opinion as to the amount of silt carried to the gulf through all the passes, but an average of the various statements made gives about 250,000,000 cubic yards per annum. This divided in the proportion of the flow through the mouths gives:

	Cubic Yards.
For the South Pass	22,500,000
" " Southwest Pass	102,500,000
" " Pass á Loutre	125,000,000

Any plan, therefore, which bottles up the natural escape for this enormous amount of sediment by two jetties and proposes to place its main reliance upon a dredging plant having a capacity much less than the annual sedimentation, must involve a herculean task, and would seem, in view of all past experiences, to be an interminable and enormously expensive problem.

On the contrary, the utilization of the energy of the stream by the interposition on the bar of a concave but stable resistance which leaves the right bank open for a natural adjustment of the regimen would appear to furnish the most economical and expeditious solution. It should be remembered also that the cross-section of the stream is more than double that required for navigation, so that if the new channel developed only 50 per cent. of the present section in the pass, it would still be more than sufficient.

CONCLUSIONS.

It would, therefore, seem to be a wise and rational policy to control the effluent stream at the Southwest Pass by a single training wall or jetty curving gently to the westward and reducing the area of discharge to that of the normal section with resulting mean depths of over 40 feet,

permitting the river to build its own counter-levee and to adjust its regimen to the new conditions without material advance of the bar seaward.

APPENDIX.

In his paper on "River Currents and Configuration of River Beds," read at The Hague International Navigation Congress of 1894, the author, L. Léliavski, a distinguished Russian engineer, said in substance:

"Parallel or nearly parallel lines look very well on the plan, but are in reality not applicable, as the idea on which they are based is in contradiction to the laws of movements of currents in rivers."

In works so constructed the channel is arbitrary and temporary, and often the reverse of what it should be.

There is no formula yet applicable to practice. The directions of water threads in a river are not parallel.

"As the curves of the bed are stronger the more remarkable is the depth produced by a great pressure of water on the bottom caused by a convergence of the water threads towards the concave banks."

"Deepening increases not with the velocity of the current, but rather with the head of water working on the deepening and especially at an increased depth. The water, by its weight, does not allow the lower strata to pass over the irregularities of the bottom, but carries them away."

In the expression

$$\frac{M}{2}(v^2 - v_1^2)$$

much depends on the difference of velocity before and after impact; between v and v_1. If the obstruction be large and the current narrow, then $v - v_1$ is large and a considerable part of the mechanical power of the water is expended on the body to cause its removal.

"Therefore, to obtain the greatest difference of $v - v_1$, the radii of curves must be shortened."

"In projecting the trace of the banks we must principally follow the configurations of natural concave banks."

"Mere narrowing is not sufficient to remove 'crossings;' the threads must be made to take '*always a convergent direction*,' thus increasing v and M and diminishing v_1.

"All the molecules of a liquid body, having different velocities, describe curved lines, not parallel and not concentric.

"The head of water in the concave bank, by its pressure, causes the water to flow at the bottom in the inverse direction and obliquely to the banks.

"Material loosened in concavities is carried by the undercurrents, which distribute it in the direction of the opposite convex banks.

"The current converging towards a concave bank engenders by its shock an undercurrent in a direction lateral to the surface.

"The currents of the rapid channel draw the more sluggish threads towards it. In this way these threads arrive at the concave banks by the force of inertia, and partly also by the result of centrifugal force.

"The currents are not due to the form of the cross-section, but, on the contrary, the formation of the profile of the bed arises exclusively from the influence of their currents.

"The converging current causes longitudinal cavities which are especially deep in the curves and the bottom current rejects the soil from the sloping banks.

"The sharper the curve of the concave bank, the more rapid is the descent of the particle (of water) from the upper beds to the bottom, the greater is the power acquired by the movement and the greater the excavation of the bottom. The depth, therefore, varies inversely as the radius of the curve.

"The depth and constancy of the navigable channel are the effect of the converging currents. There must be room for the equal divergence of the bottom currents to admit of free circulation.

"There is no converging current at the inflections of the bed; there are, so to speak, not even banks.

"In order to have a considerable and stable deepening of the bed it is necessary to give the concave bank a regular form and a sufficient curve, and by an artificial lengthening

to conduct the water across to the concavity of the next reach which has to be treated in the same manner. With this treatment the inflections (crossing bars) of the bed disappear and we obtain banks alternately concave on each side. These banks may be designated conductors of the converging water-threads and of the channel."

At this same Congress, Messrs. Mengin, Lecreulx and Guiard reported their observations of similar phenomena. They say:

"Only observation of fact is of any real value.

"It is long known that concave banks produce deep pools, and all variations in curves of banks give rise to corresponding variations in depth. Clearly shown on the Garonne River. A general formula for the relation can hardly be stated for width, depth, speed, material.

"A local increase in width causes a shoal, although upon this same shoal the speed remains great and the bottom is unstable.

"If, abandoning the rectilinear form of bed, we introduce curves, the continuity of these curves becomes extremely important."

Thus these experienced practitioners would seem to confirm in every particular the principles and practice upon which the single reactionary breakwater is based.

THE GRAPHOPHONE GRAND.

[*Being the report of the Franklin Institute, through its Committee on Science and the Arts, on the invention of Thos. H. Macdonald, and embracing a general history of sound-recording and reproducing devices, with a supplement by Mr. Philip Mauro, Washington, D. C. Sub-Committee: Louis E. Levy, Chairman; Sam'l Sartain, H. R. Heyl and J. M. Emanuel.*]

HALL OF THE FRANKLIN INSTITUTE,

[No. 2063.] PHILADELPHIA, November 28, 1899.

The Franklin Institute of the State of Pennsylvania for the Promotion of the Mechanic Arts, acting through its Committee on Science and the Arts, investigating the merits of the "Graphophone Grand," reports as follows:

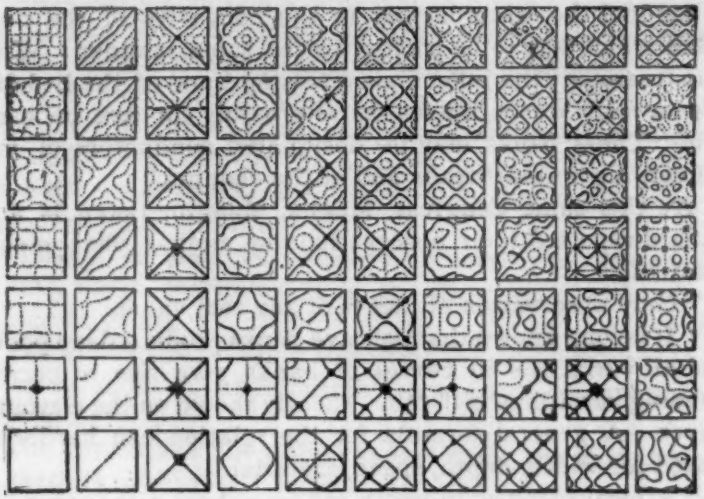

FIG. 1.—Chladni's figures.

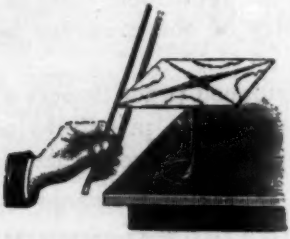

FIG. 2.

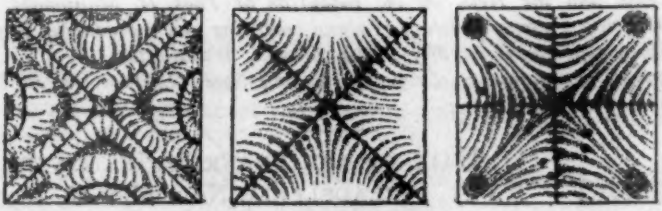

FIG. 3.

FIGS. 2 and 3.—Chladni's figures, showing symmetrical disposition of sand on plates vibrated with violin bows and dampened at various points.

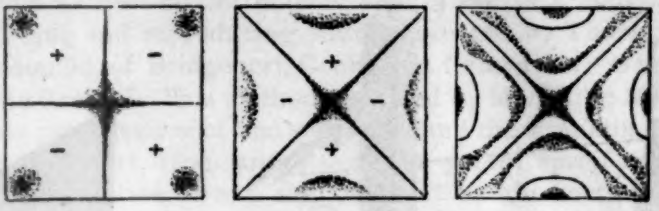

FIG. 4.
Strehlke's experiments on Chladni's plates, showing effect of mixing a very fine dust with the sand on the plates.

FIG. 5.—Wheatstone's kaleidophone.

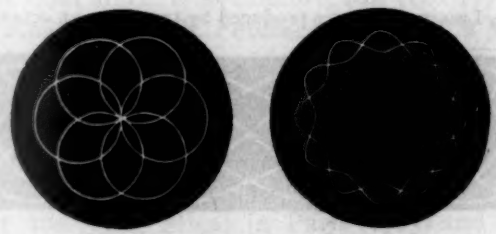

FIG. 6.—Kaleidophone figures.

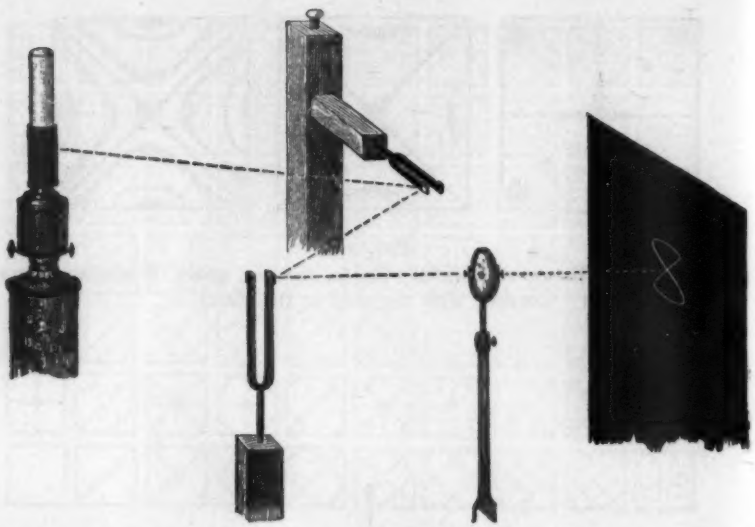

FIG. 7.—Lissajoux' apparatus for showing combined vibrations of two tuning forks at right angles with each other.

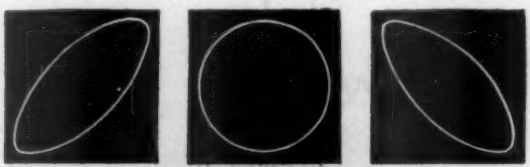

FIG. 8.—Luminous figures produced by two notes in unison.

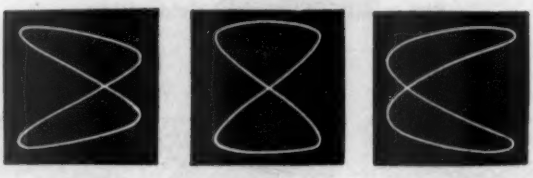

FIG. 9.—Luminous figures produced by two notes an octave apart.

FIG. 10.—Luminous figures produced by two notes a fifth apart.

The subject-matter of this application is a method of recording and reproducing sound, invented by Thomas H. Macdonald, of Bridgeport, Conn., and termed the "Graphophone Grand." This method, as stated by Mr. Philip Mauro, in his presentation of the subject before the Franklin Institute (March 15, 1899), and also in the patent specifications, is a modification of and improvement on the method patented by Bell and Tainter in 1886. A proper understanding of Mr. Macdonald's improvement, accordingly, entails a due

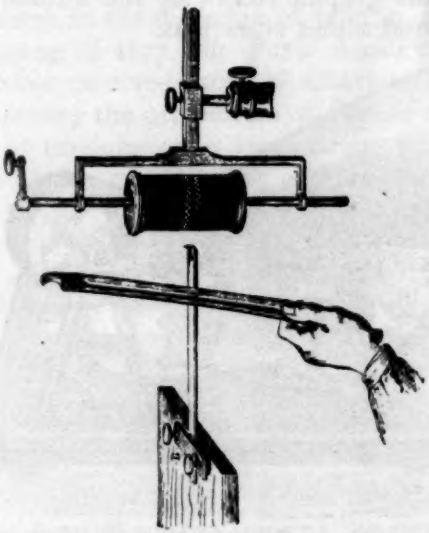

FIG. 11.—Graphic record of simple sound vibrations.

consideration of the earlier invention, and, coincidently, a review of the prior art generally.

Talking machines have been the aim of numerous inventors at least since, if not before, the year 1779, when the Imperial Academy of St. Petersburg offered a prize for the construction of a machine which should be capable of producing the vowel sounds as expressed by the human voice. The prize was awarded to Professor Kratzenstein, for his devices and investigations, which resulted in considerably widening the horizon of acoustic science, and presently thereafter the Abbé Mical in Paris (1783) and Von Krem-

pelen in Vienna (1788) were working in the same field. Many ingenious mechanisms were constructed by these workers, and improved upon by various successors, of whom Faber, of Vienna, about 1850, produced a very remarkable speech-articulating machine. In 1857, however, the attention of inventors was directed to a more promising field of effort, through the labors of Leon Scott, who patented the sound-recording machine known as the Phonautograph, in which the principles of acoustic physics were employed to produce an autographic record of the human voice and of other compound sound vibrations.

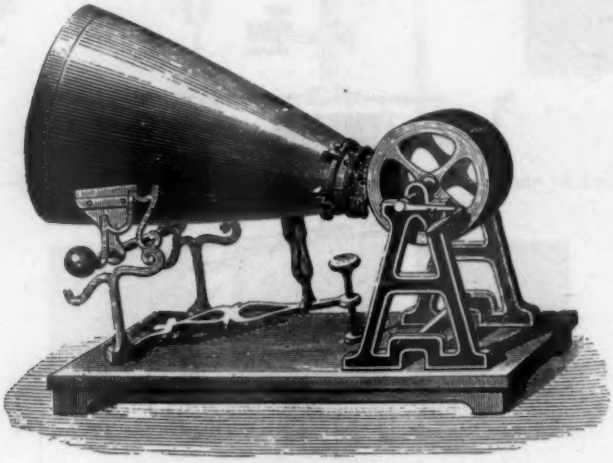

FIG. 12.—Leon Scott's Phonautograph.

The optical demonstration of rhythmic sound-waves dates back as far as 1787, when Ernst Chladni published at Leipzig his epoch-making discoveries in this field, but, although these developments were amplified by the researches of Strehlke in 1825, of Young and Wheatstone in the following decade, and by those of Tyndall, Helmholz, Melde and Lissajoux at subsequent periods, the graphic reproduction of vocal sounds appears to have been first effected by Scott's appliance. In that instrument the sonorous impulses produced by speech and other non-rhythmic tones were graphically recorded through the vibrations of a tympanum consisting of a flexible

diaphragm, acting by means of a stylus on a revolving cylinder, and it is this sensitive tympanum and its attached stylus, developed and improved by successive inventors, that form the essential elements in the talking machines of to-day.

The phenomena demonstrated by Scott were analyzed in relation to vocal sounds, and especially with regard to the separate functions of the vocal organs, by the French Linguistic Society in 1875, and these studies, together with Bell's invention of the telephone, in 1876, gave a new impetus to research in the direction of talking machines.

In the spring of 1877, the Paris Academy of Science appears to have received from M. Charles Cros a communication proposing the production of Scott's sound records in the form of tracings on a transparent surface, and the reproduction of these tracings by a photo-chemical etching

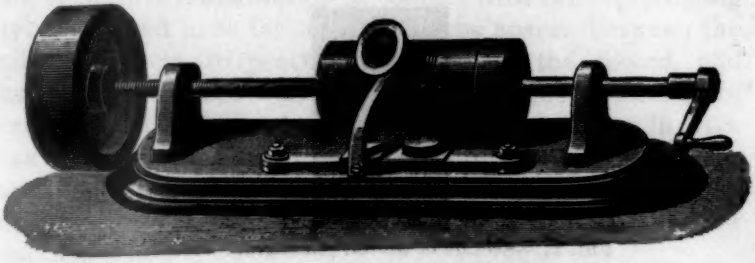

FIG. 13.—Edison's Phonograph.

process in the form of sunken lines on the record plate. In a retracing of the record, these lines were to serve as guides to the stylus, and thus the vibrations of the diaphragm, and, coincidently, the corresponding sound-waves, were proposed to be reproduced. A method analogous to this, but avoiding the intervention of photography, and producing the record directly on a plane metallic surface, was subsequently developed in this country (*infra*), but Cros appears to have gone no further than to make the communication referred to, which was not published in the *Comptes Rendus* until some six months later, in September, 1877.

In the same year, however, the actual reproduction of sound tracings back into perceptible sounds was accom-

plished by Thomas A. Edison, through his invention of the sound-recording and reproducing device termed the "Phonograph." In this instrument the movements of the vibrating diaphragm, which Scott had translated into the form of a sinuous line traced spirally around a cylinder, were reversed in their effect. Instead of the stylus being caused to move in a plane parallel with the surface of the revolving cylinder, it was made by Edison to indent the surface, and thus the sinuosities of Scott's line on an even plane were transformed into similar sinuosities of a line in a varying plane. This was done with a view to causing the stylus to retraverse the variously indented line, and thereby reproduce in the diaphragm the vibrations, and, coincidently, the original sounds, which had caused the indentations, and this effect was indeed produced in the mechanism which Edison devised for the purpose.

FIG. 14.—Section of tin-foil phonograph record.

The movements of the recording stylus being derived from the minute impulses of sound-waves acting on a membrane, Edison found it necessary, in order to produce an indentation sufficiently marked to serve the purpose of reproduction, to reduce to the utmost possible degree the resistance of the receiving material to the impact of the vibrating stylus. To this end, a thin grooved line was cut in a spiral around the receiving cylinder, and over this was fastened a sheet of tin-foil. By a suitable screw attachment the cylinder was revolved so that the spiral groove beneath the tin-foil passed under and in touch with the vibrating stylus, and thus the tin-foil was made to receive the impact of the stylus where the hollow groove in the cylinder left the foil without support. The indentations

produced in the tin-foil by the movement of the stylus were sufficiently positive and distinct to permit their translation into audible sound-waves. This was effected by causing the spiral line of indentation to repass before the stylus in contact with the latter, and thus to actuate the stylus as by a succession of cams and correspondingly to vibrate the diaphragm, which in turn transmitted its impulses to the surrounding air in the form of sound-waves corresponding to those which had produced the indentations originally. Edison's phonograph was thus the first strictly acoustic talking machine.

The very property, however, of the tin-foil, or of any similar material that afforded the possibility of its being sufficiently indented through the impulses of sound-waves, proved an insurmountable obstacle to its practical application for the purpose in view. A very few passages of the line of minute indentations in contact with the reproducing stylus sufficed to so far level down the spaces between the indentations as to practically obliterate the record, and though various means of giving permanence to the record were devised and put into practice, such as filling in with wax on the back, to stiffen it, or a reproduction of the surface by electrotyping, the technical difficulties encountered in these processes left the instrument unavailable for practical use.

Apart from various propositions to overcome this difficulty by causing a thin sheet of metal to vibrate in touch with a revolving cutting tool, in order to produce a sound record of durable quality, which does not, however, appear to have been actually attempted, the machinery of sound reproduction remained as Edison left it until 1886, when the efforts of Chichester A. Bell and Sumner Tainter, of Washington, D. C., afforded a solution of the problem. The advance effected by these two inventors consisted essentially in the substitution of a cutting edge for the indenting point of Edison's apparatus, and the coincident use of a material for their cylinders sufficiently cohesive to be practically available for reproduction and sufficiently amorphous to permit the cutting stylus to engrave its sur-

face. The substance which was found to answer these requirements best is a compound of beeswax and paraffine, which in practice was imposed on the surface of a cylinder of pasteboard. The stylus, having to cut into the surface of the recording cylinder instead of indenting it, was accordingly fixed at an angle to the radius of the cylinder, instead of parallel with it, and being held by a light spring or weight, in contact with the revolving surface, while vibrating under the impulses of the sound-waves, the vibrations were recorded in the waxy material in the form of an engraved line whose depth varied in accordance with the vibrating movements of the cutting stylus. These cuttings gave at once a record deep enough for reproduction and rigid enough to permit the repassage of the record in con-

FIG. 15.—Cross-section of Bell and Tainter's graphophone record, showing process of cutting by stylus.

tact with the reproducing stylus a considerable number of times, without the successive elevations and depressions of the record being materially altered in their relations. In this regard the smoothness of the waxy ground afforded a material advantage. The positive nature of the engraved record, and the strength of the material in which it was made, afforded also the possibility of the records being mechanically duplicated on other cylinders by transmitting, through suitable connections, the motion of the recording stylus on the former to a cutter on the revolving surface of the latter.

Apart from the great advance effected by Messrs. Bell and Tainter in producing the sound records in a comparatively permanent and practicable form, they improved in

many important respects the various mechanisms required in the process. Their apparatus, in distinction from Edison's "phonograph," was called the "graphophone."

This historical review of the progress of sound-recording and reproducing mechanisms brings us, at this point in chronological order, to the consideration of another device for the same purpose, already alluded to, and which also may be regarded as starting from the phonautograph of

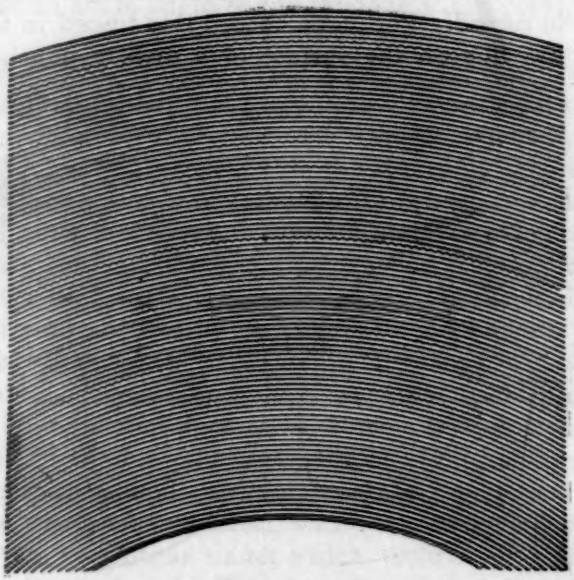

FIG. 16.—Section of Berliner gramophone record.

1857, but which, continuing on the lines of that instrument to obtain the record on a plane surface, diverged at this point and wrought this flat record into depressions by chemical means. These depressions, however, recorded the vibrations of the sound-receiving diaphragm in sinuosities of the side walls instead of sinuosities of the bottom of the lines. This was the so-called "gramophone," invented in 1887 by Emile Berliner, also a resident of Washington, D. C., and which was brought out before this Institute in May, 1888.

The principle of Berliner's procedure differs radically

from those of his predecessors, inasmuch as his record, being effected by a stylus vibrating in a plane parallel with the receiving surface, is free from the disturbances resulting from a varying resistance to its movements. This latter factor enters inevitably into the result when a depression of any surface is effected by indentation or by cutting, the resistance naturally increasing with the depth of the depression to be produced, while the latter is directly related to the amplitude of the vibration which the depression is to record. Berliner's record was traced in the form of a spiral line on the surface of a disk of polished zinc,

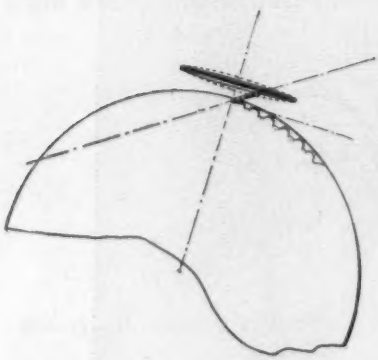

FIG. 17.—Section of graphophone record, showing condensed sinuosities.

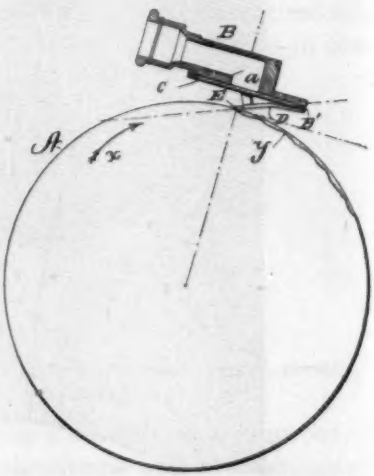

FIG. 18.—Section of Graphophone Grand record, showing extended sinuosities.

through a film of extreme tenuity, composed of a fatty acid obtained from a solution of wax. This film serves as a resistant to an etching mordant, which effects a depression of the traced line by erosion of the bared metallic surface, the etching then being ready for reproduction into sound-waves by reversing the procedure.

We now come to the latest innovation in acoustic mechanisms, the so-called "Graphophone Grand," as presented by Mr. Mauro. This invention, as has already been noted,

is a modification, by Mr. T. H. Macdonald, of the graphophone of Bell and Tainter. The improvement of the parent invention is effected (claim 1) "by imparting to the tablet a high surface speed, thereby forming undulations of great amplitude with long, gentle, easy strokes, as contradistinguished from a record of the same sound having short, abrupt undulations." Furthermore (claim 2), in the "placing of the vibratory cutting stylus in contact with a record tablet at an acute angle to the tangent at the point of contact."

With regard to these procedures, it appears from Mr. Mauro's presentation that there is no difference between the graphophone record of Bell and Tainter and that of Mr. T. H. Macdonald, except "that the recording tablet of the 'Grand' moves with a surface velocity of from two and a half to three times that given to the small tablet. The former is made large because, for practical reasons, it is preferred to obtain the high surface speed by increasing the diameter of the tablet rather than by increasing its axial speed."

With reference to these practical reasons it is noted in Mr. Mauro's presentation that "superficial considerations would indicate that such change in operation could only have the effect of producing a corresponding elongation of the undulations, and, in order to see a logical connection between the cause and effect, we must observe a little more closely the conditions under which records of sounds are made and reproduced." These conditions, it is stated, "may be roughly described as follows: The cutting stylus, performing its pendulous motion corresponding to the sound-waves, cuts a sinuous line composed of alternate elevations and depressions. As the cutting edge begins to descend to cut a depression, it at first encounters practically no resistance, the sharp edge alone being in contact with the material of the tablet; but, as the movement continues, the crest just being formed by the descent of the cutting edge comes in contact with the shank of the cutting stylus, that is, the smooth cylindrical surface forming the periphery of the stylus. As soon as this occurs a check is imposed upon the further penetration of the stylus, which is no longer free to follow

the movements of the atmospheric impulse. The effect of this check or damping influence is not merely to diminish the penetration of the stylus, but also to modify to some extent the form of the undulations.

"The effect of increasing the velocity of the recording tablet will now be easily understood. If that velocity be made sufficiently great, the crest of the undulations as they are formed are, so to speak, carried away so rapidly that they do not come into contact with the shank of the stylus at all. More exactly stated, the descending slope becomes a very gradual as distinguished from a very abrupt one, the curve approaching more nearly to parallelism with the recording surface. It follows that, the resistance or check being removed, the stylus will penetrate to a much greater depth than formerly, and will form undulations of much greater amplitude. The volume of sound being dependent upon the amplitude of the vibrations, the increased volume of sound is thus accounted for."

All this is quite true, but the utility of increasing the diameter of the cylinder to obtain the greater peripheral speed is not thereby made apparent. In a passage preceding the above quotation, it is noted that "when the speed is increased a proportionately greater amount of material is cut away in a given time, *i. e.*, more work is done and more resistance overcome during the period occupied by the utterance of a given sound. It would appear to follow that increased velocity does not diminish the resistance to the movement of the stylus, but just the reverse." It is clear enough that greater peripheral speed is desirable to the end of making the undulations of the incisions longer and correspondingly more gradual, but inasmuch as the length of the line and coincidently the extension of the undulations may be obtained on a smaller cylinder by increasing its peripheral speed, it appears certain that no gain in this respect is obtainable by the use of the larger cylinder. This conclusion is corroborated by a consideration of the element of resistance in relation to the angle of the cutting stylus to the tangent of the receiving cylinder. Immediately following the statement above quoted, Mr. Mauro notes as follows:

"The factor of resistance, however, has had an influence in determining the angle of inclination of the stylus to the recording surface and the shape of the cutting point. The latter is formed by a small cylinder of sapphire cut off in a plane nearly at right angles to the axis, so that the cutting edge is approximately an arc of a circle. With such a cutter the resistance to its action would be greatest when it operates in a position normal to the recording surface, and least when placed tangential thereto. This theoretical position of least resistance is, however, inadmissible in the operation of recording sounds. In this operation we have two movements to consider. The first is the straight-ahead movement of the tablet which results merely in the removal of the material in front of the cutting point and the formation of a groove. If the stylus were stationary during the operation it could, manifestly, and with great advantage, occupy a position almost tangential to the cylinder; but there is a second and all-important movement, namely, that due to the vibrations of the diaphragm, which movement is substantially radial of the cylinder. It would be manifestly impossible for the stylus to perform this movement at all if it occupied the ideal tangential position, for that would imply forcing the stylus sidewise into the hard body of the tablet, a task far beyond the strength of the feeble sound-waves. In practice, therefore, the stylus has been inclined at an angle of about 35° to the tangent at the point of contact."

Thus, it appears that the additional work imposed on the cutting stylus by the elongation of the line which it must cut is overcome by giving the cutting tool a sharper angle of incidence and possibly a sharper cutting edge. The added work entailed on the cutting stylus by the increased depth of the depression, obtained by freeing its shank from the back-hold of the preceding elevation, may be left out of account as being equivalent in work of the power lost in the back-hold. We have, therefore, to consider only the final element of the subject here presented, the position of the cutting stylus in relation to the tangent of the receiving cylinder.

It is plain (1) that the most efficient position of the cutting stylus would be in what Mr. Mauro recognizes as the ideal, that is, the tangential position; (2) that the smaller the diameter of the cylinder, the more nearly could the tool be brought to the tangential position without suffering the hold-back of the ridges in effecting the desired impressions.

Conversely, the highest position, that is, the largest angle of incidence required for freedom from back-hold, would be necessitated if a flat surface were to be incised by the tool, and inasmuch as the surface of a large cylinder offers more nearly the geometrical condition of a plane

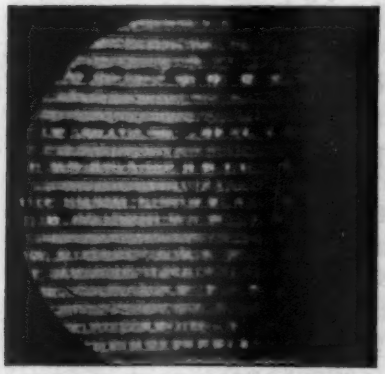

FIG. 19.—Reduced reproduction of ordinary graphophone record.

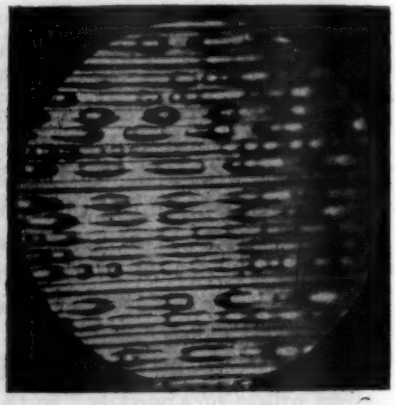

FIG. 20.—Reduced reproduction of Graphophone Grand record.

than that of a small cylinder, it follows that the efficiency of a cutting stylus, depending as it does on the acuteness of its angle of incidence, would be greater on a small than on a larger cylinder.

The possibility of obtaining a greater volume of sound by enlarging the characters of the sound record is noted by Berliner, in the *Journal of the Franklin Institute* for June, 1888. Mr. Mauro's statement that "methodical experiments have shown that beyond a certain critical speed there is no gain either in loudness or quality, but that,

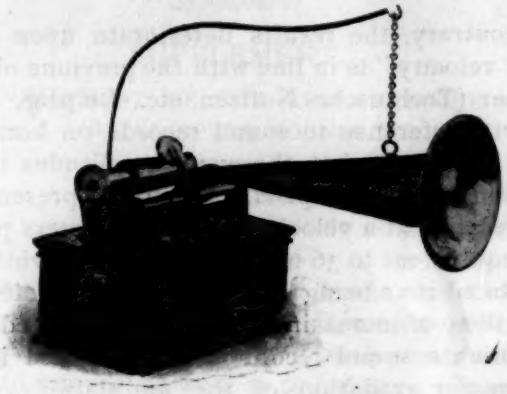

FIG. 21.—The graphophone.

FIG. 22.—The Graphophone Grand.

on the contrary, the results deteriorate upon further increase of velocity," is in line with the previous observations of Berliner (Technische Notizen, etc., Leipzig, July, 1890), where, with reference to sound records on horizontal surfaces, it is stated "that the wave amplitudes increase up to a certain peripheral speed. Up to the present time this limit is reached at a velocity of 60 centimeters per second." This is equivalent to 36 meters per minute, while Mr. Macdonald stated it as being approximately 44 meters.

The utility of increasing the peripheral speed of a cylinder in which a sound record is to be incised is apparent in the greater gradation of the undulations and in the freedom from back-hold of the cutting stylus which these more gradual undulations afford. The embodiment of this utility in the Graphophone Grand is hereby recognized by the Franklin Institute, through the award to Mr. Thomas H. Macdonald of a Certificate of Merit.

In view, moreover, of the material advance effected by Messrs. Chichester A. Bell and Sumner Tainter in the development of sound-recording and reproducing mechanism, through their invention of the graphophone, the Franklin Institute recommends the award of the John Scott Legacy Premium and Medal to the said inventors jointly.

Adopted at the stated meeting of the Committee on Science and the Arts, held Wednesday, January 3, 1900.

JOHN BIRKINBINE, *President.*
WM. H. WAHL, *Secretary.*

Countersigned by

EDGAR MARBURG,
Chairman Committee on Science and the Arts.

SUPPLEMENT.

RECENT DEVELOPMENT OF THE ART OF RECORDING AND REPRODUCING SOUNDS.*

By Philip Mauro,
Member of the Institute.

In the course of the commercial development of the art of recording and reproducing sounds, a notable result has recently been produced, which is of such a nature as to be of interest to science as well as to industry. It gives me great pleasure to present a brief explanation touching this new departure before this Institute, which has done, and is doing, so much for the encouragement of science and the useful arts.

The theory of making graphic representations of acoustical vibrations has long been familiar to physicists, but the practical application of that theory to the reproduction of audible copies of the original sounds was not brought within reach until the invention of the electric speaking telephone, in 1875, by Alexander Graham Bell.

The close relation between the telephone and the graphophone is obvious, and, as a matter of history, it was the existence of the former that suggested the first attempts at the reproduction of sounds from a prepared register of acoustical vibrations.

In the year 1877 Mr. Thomas A. Edison was occupied with experiments looking to the improvement of the telephone, and was one day testing with his finger the force of the vibratory movements of a telephone diaphragm. It then occurred to him that the characteristic form of these vibrations might be registered upon a pliable material by means of a point attached to the diaphragm. So far there was nothing original in his thought, for Leon Scott had, many years previously, devised an instrument (known to all physicists as the "phonautograph") whereby graphic representa-

* Read at the Stated Meeting of the Franklin Institute, held Wednesday, March 15, 1899.

tions of sonorous vibrations could be traced by means of a stylus in a film of lampblack, the stylus being carried by a vibratory diaphragm. But Mr. Edison's mind went further, namely, to the utilization of this registration or "record" to vibrate the same or a similar diaphragm, and thus produce sounds similar to those by which the graphic representation was made. The outcome of this idea was the tin-foil phonograph, with which every one is familiar, and which in the year 1878 attracted much attention, and gave rise to great expectations. To describe this instrument would be superfluous, but in order to give to later as well as earlier events their true significance, one or two facts with reference to it should be noted.

The method adopted was, theoretically, perfect; that is to say, the tin-foil phonograph was an attempt to embody the true conception that, if a diaphragm could be made by mechanical means to copy, even on a much reduced scale, the motions performed at a previous time by the same or a similar diaphragm when actuated by sonorous vibrations, reproductions of such vibrations would be attained. This theory was by no means new to physicists; but, prior to 1876, no mechanism capable of operating in conformity to it had been devised. What the telephone did for the kindred graphophonic art in that year was, mainly, to reveal the fact that a very small fraction of the force of the original sound-waves would suffice to actuate the repeating diaphragm, and to produce distinctly audible and recognizable sounds.

This was a fact of the most profound significance, and not likely to have been discovered by *a priori* reasoning. The force of acoustical vibrations is exceedingly feeble, and when attenuated by transformation (as in the telephone) into electrical energy, and back again into mechanical vibration, it is difficult, even with the evidence of our senses, to realize that the small residuum of energy conserved to the end of the series of operations is sufficient to give rise to audible sounds. This discovery demonstrated that the vibratory diaphragm could be made to do a certain amount of *mechanical work*, without such loss of energy

and without such distortion as to defeat the object of audible reproduction of the original sounds.

Mr. Edison availed himself of this newly discovered fact in his tin-foil indenting phonograph; and the desired and expected result was realized to the extent that sounds *were* produced with *sufficient loudness to be audible;* but disappointment was met in the fact that, somewhere in the series of operations, the characteristic *forms* of the vibrations were so lost or distorted that the reproduced sounds were but caricatures of the originals, and the reproduction of recognizable spoken utterance was not realized to a useful extent.

The questions how, where and why this loss and distortion occurred were extremely difficult of solution; and the literature of the subject, at that time, reveals no attempts to analyze or explain the result. At a much later day, and in the light which subsequent discoveries threw upon the subject, it became possible to understand why the Edison method of indenting or embossing a pliable material was and is incapable of accomplishing its intended object. The defects were of several sorts, but it is sufficient for our purpose to notice the main cause of failure, which was inherent in the method itself and quite independent of its mechanical embodiment.

For theoretical success this method required that the recording material be *perfectly mobile*, and capable of yielding, like a liquid, to all the complex movements of the recording stylus. On the other hand, in the operation of "reproducing," the material is required to have *sufficient rigidity* to actuate the stylus and diaphragm. Hence, the better a material answered to the requirements for *making* the record, the more unfit it was for *reproducing* the recorded sounds.

The failure of the phonograph was so pronounced as to discourage effort in the same direction for a long period of time. From 1879 to 1886 the literature of the art reveals no serious attempt to accomplish the reproductions of sounds, and no advance whatever was made during that period. Those were the seven years of famine in the art.

I have shown how, in one sense, the graphophone resulted from the telephone. I have now to show how this was true in another, and quite different, sense.

One of the acts for which the Emperor Napoleon I is commended even by his severest critics was the institution of the Volta prize, to be awarded by the French Government, upon the recommendation of the French National Academy of Sciences, for discoveries of great importance in electrical science. This prize was awarded in 1880 to Alex. Graham Bell for the discovery of the telephone; and he conceived the happy idea of appropriating the money part of the award to forming an association whose serious work should be the advancement of acoustical science. To this association, composed of himself, Dr. Chichester A. Bell and Mr. Charles Sumner Tainter, he gave the name "Volta Laboratory Association," in commemoration of the award.

The associates labored earnestly from 1881 to 1885, and as the result of their labors made many valuable contributions to science. Foremost among these was the method of recording and reproducing sounds, now in universal use, by *engraving a solid material of amorphous character*, such as wax or wax-like compositions. This system as a whole embodied many discoveries and inventions which con tributed to the desired end and which cannot be referred to in detail within the limits of our available time. To mark the contrast with the embossing process it will suffice to notice (1) that, in the engraving method, the point of the stylus, which is a veritable graver's tool, is constantly *embedded in* the recording material, instead of merely *pressing on its surface*. Hence it acts constantly, as well during the backward as during the forward movement of the diaphragm, producing a *continuous* record without skips or breaks; (2) that the stylus removes the material by cutting it away, instead of merely displacing it, and that it does not disturb the material except at the *exact point of contact*; whereas the embossing stylus produced a depression and disturbance extending over a considerable area around the point of contact; (3) the engraving method resulted not only in accurate and recognizable records, but in records

that could be *removed from the machine*, handled and transported without detriment, and which could be used scores, indeed hundreds of times. This last-mentioned characteristic of the new graphophonic sound record is of the very first importance from the industrial point of view. Such was the graphophone as patented by Dr. Chichester Bell and Mr. Tainter in 1886.

Thirteen years have now elapsed, during which this great discovery has passed from the laboratory to the factory, and has become the foundation of an important industry. The incentive to improve the operation has induced inventors in large numbers to attempt it, and capitalists to lend their financial aid. Yet, save in details of mechanism and refinements of various sorts, everything remains as Dr. Chichester Bell and Mr. Tainter left the system in 1886. The main effort during this period has been for *greater volume of sound*. In this direction the art has been painfully struggling for many years, and the results have seemed to indicate that a practical limit had been reached.

But within a few months a new development has taken place, which produces results in volume of sound and in fidelity to the original, far exceeding the limits of what was previously, and by those best able to form an opinion, deemed possible.

What is of peculiar interest in this connection is the fact that the result in question is produced by an apparently slight variation from previous practice, and the attempt to trace the observed result from the apparent cause will form the concluding portion of my remarks.

It is appropriate at this point to offer a practical demonstration in order that your ears may judge of the extent of the advance that has been made. I shall first show you the result that has been attained by the standard graphophone, and from which a comparison can be made.*

The difference observable between the two machines before you (overlooking unimportant details of construc-

* A demonstration was here given, using a record made by the old process and one of the same selection made by the new process.

tion) is that the cylindrical tablet of the loud-sounding machine is *larger* than that of the other. No one in this audience will for a moment suppose that the volume of sound is in any way affected by, or dependent upon, the *size* of the recording tablet. It would be as reasonable to suppose that characters written on a large piece of paper would be visible farther than characters of the same kind written on a small piece of paper. The other mechanisms are substantially the same in the two cases, that is to say, the same recording device is used, the same material for the tablet, and the same reproducer. The difference is that the recording tablet of the "Grand" moves with *a surface velocity of from two and a half to three times that given to the small tablet.* The former is made large because, for practical reasons, it is preferred to obtain the high surface speed by increasing the diameter of the tablet rather than by increasing its axial speed.

The known conditions, therefore, lead us to inquire how the mere increase in speed can bring about so remarkable a result. Superficial consideration would indicate that such change in operation could only have the effect of producing a corresponding elongation of the undulations; and in order to see a logical connection between the cause and effect we must observe, a little more closely, the conditions under which records of sounds are made and reproduced.

An important factor here is the *resistance* which the substance of the tablet offers to the action of the cutting style, which will conform absolutely to the movements of the acoustical vibrations only if absolutely free and unimpeded. This resistance is very slight, owing to the properties of the substance employed, which is an insoluble soap, and to the fact that a very sharp jewel point is used, whose normal penetration is not much more than one-thousandth of an inch, so that the style has to cut away an exceedingly small amount of a material having very slight cohesion. When the speed is increased a proportionately greater amount of material is cut away in a given time, *i. e.*, more work is done and more resistance overcome, during the period occupied by the utterance of a given sound. It would appear to

follow that increased velocity does not diminish the resistance to the movements of the style, but just the reverse.

The factor of resistance, however, has had an influence in determining the angle of inclination of the style to the recording surface and the shape of the cutting point. The latter is formed by a small cylinder of sapphire cut off in a plane nearly at right angles to the axis, so that the cutting edge is approximately an arc of a circle. With such a cutter the resistance to its action would be greatest when it operates in a position normal to the recording surface, and least when placed tangential thereto. This theoretical position of least resistance is, however, inadmissible in the operation of recording sounds. In this operation we have two movements to consider. The first is the straight-ahead movement of the tablet, which results merely in the removal of the material in front of the cutting point and the formation of a groove. If the stylus were stationary during the operation it could manifestly, and with great advantage, occupy a position almost tangential to the cylinder. But there is a second and all-important movement, namely, that due to the vibrations of the diaphragm, which movement is substantially radial of the cylinder. It would be manifestly impossible for the stylus to perform this movement at all if it occupied the ideal tangential position, for that would imply forcing the stylus sidewise into the hard body of the tablet, a task far beyond the strength of the feeble sound-waves. In practice, therefore, the stylus has been inclined at an angle of about 35° to the tangent at the point of contact.

Under these conditions, what takes place in the operation of making a sound record may be roughly described as follows: The cutting stylus, performing its pendulous motion corresponding to the sound-waves, cuts a sinuous line, composed of alternate elevations and depressions. As the cutting edge begins to descend to cut a depression, it at first encounters practically no resistance, the sharp edge alone being in contact with the material of the tablet; but as the movement continues the crest just being formed by the descent of the cutting edge comes in contact with the

shank of the cutting stylus, that is, the smooth cylindrical surface forming the periphery of the stylus. As soon as this occurs a check is imposed upon the further penetration of the stylus, which is no longer free to follow the movements of the atmospheric impulse. The effect of this check or damping influence is not merely to diminish the penetration of the stylus, but also to modify to some extent the form of the undulations.

The effect of increasing the velocity of the recording tablet will now be easily understood. If that velocity be made sufficiently great, the crest of the undulations as they are formed are, so to speak, carried away so rapidly that they do not come into contact with the shank of the stylus at all. More exactly stated, the descending slope becomes a very gradual as distinguished from a very abrupt one, the curve approaching more nearly to parallelism with the recording surface. It follows that the resistance or check being removed, the stylus will penetrate to a much greater depth than formerly, and will form undulations of much greater amplitude. The volume of sound being dependent upon the amplitude of the vibrations, the increased volume of sound is thus accounted for.

Methodical experiments have shown that, beyond a certain critical speed, there is no gain either in loudness or quality, but that, on the contrary, the results deteriorate upon further increase of velocity.

The improvement in quality resulting from this new method is apparently due to the fact already stated, namely, that the diaphragm and cutting stylus are at all times free to move in accordance with the acoustical vibrations. The test of the ear convinces us that the records made by this process correspond very closely to the form and amplitude of the actuating vibrations. But in all probability they correspond more closely than that test would indicate, for the conclusiveness of that test would imply a perfect-acting reproducer. It is appropriate, therefore, to notice the construction of the reproducer and the conditions under which it operates. It will appear from this examination that the operation of reproducing the recorded sound is materially affected by the invention under discussion.

The reproducing stylus has an end of spherical curvature which rides in the record groove, being automatically held therein by the weight of the mounting of the reproducer, which is supported on a universal joint.

The object sought is to give to the device such weight that the stylus will at all times rest in the record groove and follow accurately the undulations thereof. With the low-speed record it could not do this. The crests, when very close together (as in high-pitched sounds), did not afford the point (which must have a relatively large area in order not to cut the record) opportunity to descend fully into the intervening depressions. Moreover, when an abrupt ascending slope impinged against the reproducing point, the effect was that of a blow, throwing the point away from the record and causing an interruption of the operation. With the gentle slopes and wide curves of the high-speed record this irregularity is largely eliminated, and we approach much nearer to the theoretical condition for perfect reproduction, namely, that the diaphragm should be at all times controlled by the sound record, and should move in strict accordance with the form of its undulations. Not the least remarkable and interesting fact connected with our subject is that the discovery of a relation between the velocity of the recording material and the volume and quality of the reproduced sounds has so long escaped attention. It appears strange indeed that, with so many observers and with so strong an incentive to increase the volume of sound, this simple law has not sooner been discovered. It seems, however, that variations of speed within ordinary limits produce no noticeable difference, and obviously, other things being equal, it is desirable to use as low a speed as practicable, and thus obtain a record of maximum length upon a surface of given area.

The credit for this interesting discovery and for its embodiment in operative mechanism is due to Mr. Thomas H. Macdonald, of Bridgeport, Conn.

ELECTRICAL SECTION.

Stated meeting held Tuesday, April 24, 1900.

ELECTRICAL MEASURING INSTRUMENTS.

By J. Franklin Stevens.

In attempting a brief discussion of so broad and comprehensive a subject as that assigned me for this evening, I have been principally concerned in trying to decide just what portion of the general subject I should touch upon and in what manner I could treat that portion briefly, for I cannot hope to be comprehensive, and yet have my remarks present a semblance of coherence.

Since we have available a very extensive literature bearing on the subject of laboratory testing instruments, in which can be found descriptions of the instruments employed, their construction, application and approximate laws, I shall pass over that portion of the subject and confine myself to commercial direct reading indicating instruments, such as are to be found in every-day use.

The history of the art embodied in the commercial manufacture of indicating instruments is extremely interesting, but shows many analogies to the development of other branches of electrical engineering, with the single exception that practical data in published form is extremely scarce. Every one entering this field is compelled to master some of the most intricate problems known to the profession, and must learn for himself how best to apply theoretical deductions to practical mechanical devices. Until very recent years, our sole guide has been the study of early and primitive types which in their time were useful and valuable, but which to-day fail to reach the high standard we have learned to demand of an instrument whose function is to indicate the input or output of our many and varied electrical devices. Every new device put before the public has meant a new condition to be met by the instrument manu-

facturer, requiring of him a new design or modification of an existing design. Two rather recent examples of this occur to me: one, the increasing tendency to employ high voltages in commercial work, reaching frequently 12,000 volts in the case of series constant-current arc light systems and 33,000 volts in long-distance power transmissions; the other, the rapid development of the electric automobile, which requires a volt-ammeter of as high a degree of accuracy as the portable laboratory instrument, yet able to withstand the roughest usage ever accorded an instrument of precision, coupled with restrictive specifications as to size and weight. In every instance instrument manufacturers have met the demands made upon them better, I think, than many of our fellow-workers have in some other lines. Much yet remains to be done, and much, I am confident, will be done in the matter of perfecting existing instruments in the near future. Gradually inefficient or unreliable types are being relegated to oblivion, and the remaining ones brought closer and closer to standard uniform construction.

The greater number of measurements required to-day can be made with a voltmeter, ammeter and wattmeter, and, while it might be thought that the wattmeter is merely a combination of the voltmeter and ammeter mounted in one case, this is not true in all classes of measurement. While it is true that the reading of the wattmeter represents the product of the readings of the voltmeter into the ammeter in the case of direct current measurements, it is not true in the case of alternating current measurements, save in the very exceptional case of a circuit possessing neither capacity nor inductance or a balance of the two. When either inductance or capacity exist there is a consequent lag or lead of the current wave relative to the pressure wave, and, since the instantaneous readings of the volts and ampères represent the mean effective or virtual values, that is, the square root of the mean square, their product differs from the reading of the wattmeter, which indicates the integrated values of two curves, the maximum and minimum values of which occur at different times. To obtain an agreement

between the values obtained by volt-ampère readings and watt readings, it is necessary to multiply the volt-ampère readings into the cosine of the angle of lag or lead, so that the ratio of the watt readings, as given by a properly constructed wattmeter, to the volt-ampère readings gives us the power factor of the circuit, and from the two sets of readings we can readily determine the angle of lag or lead and the so-called wattless current. Probably this is well known to all of you, yet scarcely a week passes but I have to explain the matter to one or more customers who cannot reconcile the difference in the two sets of readings and are inclined to believe that some, if not all, of their instruments are indicating erroneously.

Before touching on the different types of indicating instruments which are most commonly found in commercial use, let us consider for a moment the essential requirements of a practical and satisfactory instrument for measuring voltage, current or power. To start with, the instrument must be direct reading. We have no time to-day to convert angular deflections into true values by means of tables of constants, but insist that the pointer shall indicate directly in definite units the value of the passing load or the impressed electro-motive force. We must next be assured of the accuracy of the calibration, and for that depend principally upon the reputation of the maker or check the indications by comparison with a secondary standard. Beyond this we must know that the accuracy of the indications is not affected by the influence of ordinary external fields or by normal variations of temperature, due either to heating within the instrument, by means of the passage of the current to be measured, or to variations in the temperature of the room in which they are installed. All instruments should be mounted in dust-proof cases, and the systems carried in jewelled bearings; the movement of the pointer should be aperiodic or dead-beat, and, so far as possible, the system should contain nothing subject to change or deterioration, and should be constructed so that it will withstand successfully the ordinary rough usage liable to be accorded it in practice. The question of gradual change of accuracy,

due to the change in some constituent part of the system, has led me to strongly advocate the use of electro-magnetic instruments with gravity control for switchboard use whenever the conditions render them applicable, and I fully expect to see this type of instrument become more popular as its advantages become more widely appreciated. Such a system is not applicable for portable instruments, save the dynamometer system for alternating currents, and even then a spring control must be used; nor is the series electro-magnetic ammeter practical above about 1,500 ampères, due to the structural difficulties involved in carrying the bus bars directly to the instrument terminals. The one objection most commonly urged against the electro-magnetic instrument for direct current switchboard use is the unequally divided scale, for, as a rule, the scale only covers a range starting at 10 per cent. of the total up to maximum. In most cases, however, this objection is purely captious, for a switchboard voltmeter is seldom used to indicate more than 10 per cent. above or below the normal voltage, and the ammeter indications are of principal importance in the higher ranges to prevent a possible overload of the circuit or generator. In alternating current circuits an equally divided scale is an impossibility, so why should we sacrifice constancy in direct current measurements for the sake of an equally divided scale when the lower registers are so seldom used? In portable direct current instruments the proposition is somewhat different, as we desire to cover the maximum limits of possible measurement on a single scale instrument; therefore, an equally divided scale is desirable and practically necessary.

Taking up now the different types of instruments which are in most common use, we find they may be roughly divided into six classes, depending on the principle by means of which measurements of voltage, current or power are made: (1) the hot-wire; (2) the tangent galvanometer; (3) the electro-static; (4) the dynamometer; (5) the D'Arsonval galvanometer and (6) the electro-magnetic.

The hot-wire instrument is designed to operate by the expansion of a fine wire or strip of conducting material

due to the heating produced by the passage of the current to be measured, and, therefore, the indications are proportionate to the square of the current. This type of instrument possesses the advantage of indicating with equal accuracy the current flow on either direct or alternating current circuits and is independent of the frequency of the circuit or of the shape of the current wave; it is, further, unaffected by the presence of external fields. These advantages, however, are all that can be claimed for this system, and to offset them are many serious disadvantages. Since the instrument operates by virtue of temperature differences, it is extremely susceptible to variations of external temperature, and such variations must be compensated either by mechanical means, which provide for bringing the pointer to zero by increasing or decreasing the tension on the working wire, or by providing auxiliary heating strips which are supposed to automatically adjust the tension of the working wire on the general principles of a thermostat. This may seem a small matter to most observers, but to me it signifies an error in principle, and reminds me of one of our early types of arc dynamos which, by reason of incorrect construction, was found to spark badly; so, in place of correcting the design, an air blast was provided to blow out the spark. Another point of objection is the constant change of zero, due to the fact that until the wire has been stretched to its elastic limit it will fail to return to the same point after having been stretched by the heat applied and will always show a slight residual strain. To attempt to correct this prior to installation in the instrument means, judging by my own experience, that at least 90 per cent. of the wires will break during the preliminary stretching process, and the remaining 10 per cent. will stand but a very slight overload in service. As overloads are quite common, this would constitute a serious objection, for it means the return of the instrument to the manufacturer for the insertion of a new wire and recalibration. Another very serious objection is the amount of current required to operate the instrument. The average resistance of a modern hot-wire voltmeter is 4

ohms per volt, which means a current flow of ·25 ampère. This low resistance effectually bars this type of instrument for accurate testing, since the amount of power abstracted from the circuit to be tested vitiates the accuracy of the results obtained and introduces serious errors, which must be allowed for in all calculations. Recently an attempt has been made to introduce the hot-wire instrument as a shunt ammeter, particularly for alternating current measurements. As a shunt ammeter, the hot-wire instrument is a distinct failure, for, while the manufacturers claim it can be operated on a drop of ·3 volt for full scale, my personal experience has shown a drop of 6 volts to be necessary. On the makers' figures, 300 watts would be required to operate a 1,000 ampère ammeter, nearly ·4 horse-power per instrument, while on the sample I have tested 6,000 watts, or 8 horse-power, would be required. Again, it has been shown that these ammeters when calibrated on direct current are not accurate within 10 per cent. when used on alternating currents, unless the reactance in the shunt is exactly equal to the reactance in the instrument, a condition almost impossible to obtain, even with the most careful design.

A modification of the hot-wire system has been proposed, consisting of an enclosed chamber containing a fixed resistance wire, one end of the chamber, which is made air-tight, consisting of a flexible diaphragm operating the pointer through a system of levers. In this case, the heat supplied to the wire by the passing current causes the air in the enclosed cylinder to expand and, consequently, puts the diaphragm under tension, very much on the principle employed in the aneroid barometer. I find by actual experiment that such a system could be applied to voltmeters, and could be so made that it would be accurate in its indications and would possess a reasonably high resistance. The feature, however, which renders this system impracticable is the slowness with which the readings can be taken. The pointer will not come to rest until after the heat supplied the air enclosed within the chamber is equal to the heat radiated by the walls of the enclosing chamber, and from

one and a half to two minutes are required before the pointer will come finally to rest at the part of the scale marked to indicate the impressed E.M.F. This is true no matter how small the air chamber is made; and while it is possible to shorten this time by carefully covering the sides and one end of the cylinder by a heat insulating jacket, still it cannot be constructed so as to give an instantaneous reading, and every attempt to shorten the time by covering the walls of the chamber results in a corresponding increase of the time required for the pointer to come back to zero after current has been shut off. In other words, the instrument is similar in its action to an ordinary thermometer, which, as you know, cannot be made to respond instantly to changes of temperature.

Electro-static instruments, which depend for their action on the mutual attraction of two plates connected to the opposite sides of the line, are still used, but not extensively, for high tension measurements. They will indicate correctly on either direct or alternating current circuits, but are limited to measurement of voltage. It is impossible with this type of instrument to obtain a low reading, as the attraction of the plates toward one another varies approximately with the square of the potential difference between them. I say "varies approximately" advisedly, as the capacity of two plates varies directly with the square of the voltage only when the distance between the plates is maintained constant, which in this case is an obvious impossibility. As a rule, these instruments cannot be made dead-beat without introducing elements liable to seriously interfere with their accuracy; and, while there are no errors due to temperature changes, and while they are also extremely efficient, requiring no current flow at all, they are affected very considerably by external influences, particularly by static charges, which are almost invariably present in power stations or dynamo rooms, and, as you probably know, it is practically impossible to provide a shield for static effects.

Instruments employing the principle of the tangent galvanometer are still employed, and for certain classes of

measurements are extremely useful. They can be particularly recommended for use as ground detectors or differential voltmeters, where the actual value of the indicators is of small moment, the particular object being to show a balance of two opposing voltages or the presence of a ground by deflection. In this system the magnetized needle is suspended vertically, and is acted upon by two equally and oppositely wound solenoids; that is, the solenoids are wound with two wires in multiple and then cross-connected, so that we have two circuits equal in effect but opposite in action. I might say in passing, that if the proper proportions of solenoids to magnetized needle are employed, and the length of the magnetized needle properly proportioned to its area, the tangent galvanometer instrument may be made extremely accurate and also quite permanent. Care must be taken, however, that under no conditions can the direction of the lines generated by the solenoids oppose the lines existing in the magnetized needle, otherwise the value of the calibration is almost instantly destroyed.

For measurements of alternating current voltage, and also for the measurement of the watt output of any alternating current source of energy, the dynamometer system, in one of its many modifications, is unquestionably the best type of instrument that can be used. This instrument indicates directly the mean effective voltage of the line to which it is connected, and is equally accurate for direct current measurements, provided readings are taken with the direct current flowing through the instrument in one direction, then the direction of the current flow reversed and a second reading taken, the mean of the two readings being the correct value of the impressed electro-motive force. This for the reason that the instrument is extremely susceptible to the influence of external fields. Further, the dynamometer type of instrument is only properly adapted for use in portable instruments. The fact that two readings are necessary on direct current prohibits its use as a switchboard instrument on direct current circuits, and its delicacy, coupled with its field of low intensity and its sus-

ceptibility to error, due to the presence of iron in its immediate neighborhood, whether used on direct or alternating current circuits, renders it impracticable as a switchboard instrument for alternating current circuits. As an ammeter, it cannot be successfully employed for measurements above ·5 ampère, due to the difficulty of providing perfectly flexible contacts capable of carrying large currents. When you attempt to convey a current into a moving coil beyond the carrying capacity of a flexible conducting spring, recourse must be had to mercury contacts, and a mercury contact should never be employed in a portable or enclosed type instrument. In the dynamometer instrument great care must be taken to have the moving coil as light as possible; and to render the instrument aperiodic, or dead-beat, an aluminum air vane, moving in a partially enclosed chamber, must be employed. Manually operated brakes acting on the delicate moving system should be avoided, as their continued use results in a serious derangement of the moving parts.

Instruments based on the principle of the D'Arsonval galvanometer are very extensively used to-day for both switchboard and portable instruments on direct current circuits; they can be calibrated so as to show an equally divided scale, and, if properly constructed, are an extremely efficient instrument. Further, they are very well adapted to the measurement of resistances and grounds. In the construction of this type of instrument the important factor is the permanency of the permanent magnet fields employed. It is essential, in the first place, that the proper grade and quality of steel shall be employed, which means a steel possessing high permeability and a high degree of retentivity; then this steel must be carefully worked within definite limiting temperatures and must be treated with great care and skill in the processes of magnetizing and ageing. Further than this, the moving coil should be extremely light and the whole system carefully shielded from the influence of external fields, and yet so shielded that the moving coil will not be robbed of an appreciable number of lines of force, due to the presence

of the shielding iron. The greatest field for this type of instrument is in the measurement of heavy currents; that is, for currents of 1,000 ampères and upwards, due to the fact that it can be operated as a shunt instrument, requiring an extremely small drop of potential, full scale being obtained with a drop of from ·03 to ·05 volt. This means a relatively high efficiency, and, while the instrument does not possess all of the advantages of a series ammeter, yet it is far easier to install, and, where a high degree of accuracy in current indications is not demanded, will be found very satisfactory in practice.

Further than this, the D'Arsonval instrument, which gives deflections directly proportionate to the current flow, lends itself readily to a large variety of measurements, such as determining resistances and measuring the drop of potential from which resistances and grounds may be readily computed. It is, however, a type of instrument which should be handled with great care, as rough handling or the presence of powerful external fields will permanently destroy the accuracy of its indications. Further, since it depends primarily for its continued accuracy on the maintenance of a field of uniform strength, as supplied by its permanent magnet field, instruments of this type should be frequently checked to ascertain whether the permanent magnets have maintained their initial strength.

A great deal might be said on the subject of the last type under consideration, namely, the electro-magnetic system, for there are probably more variations in practical construction contained in this type than in all the other types put together; some are good, many very bad. It is, however, unfair to adopt the policy of certain engineers, who unqualifiedly condemn every electro-magnetic instrument, solely and entirely on the grounds that they contain a moving mass of iron, which, in their opinion, must render the instrument subject to errors of lag and hysteresis. My practical experience with this type of instrument, covering a great many years, has taught me that it is perfectly possible to so construct an electro-magnetic instrument containing a mass of moving iron that errors of lag and hysteresis,

if they are present, are so small as to be negligible. In order to achieve this result it is necessary, first, to proportion every part of the instrument with reference to all other parts; to carefully shield the instrument from the influence of external fields, and this by means of a shield that will not introduce errors due to its own retentivity. The iron employed should be very small and very light, and should be selected, after careful test, for purity and absence of retentivity. It should then be formed up with care, and so treated that oxidation is practically impossible. The field due to the actuating solenoid should likewise be carefully studied, and should be designed so that the moving iron will at no time be completely saturated, and yet of sufficient strength, in conjunction with the shield employed, as not to permit the indications of the instrument to be seriously influenced by external fields. In no case should two masses of iron be employed, as is common in some types of instruments, which depend on the repulsion of a moving vane by a fixed vane of similar polarity, both being energized by the actuating solenoid. It is almost impossible to free double vane instruments from errors of lag or hysteresis, and there is a further tendency for the pointer to stand off zero, due to the residual magnetism mutually induced in the two vanes.

The fact that the instruments constructed on the electromagnetic principle contain no material subject to change or deterioration, coupled with the fact that they can be built solidly and substantially, and controlled by gravity in place of a spring or springs, renders them exceedingly reliable in practice. If their calibration is correct when first installed, there is no reason why the calibration should change with time.

In the above brief description of the most common types of indicating instruments, it may be noted that there is no one type which is universal, that is adapted for both switchboard and portable use for either direct or alternating current measurements. While we can very readily dispense with the hot-wire and electro-static instruments, the other four types are essential for some class of measurement, and

for any particular line of measurement the most suitable system should be selected. It is possible that some day a universal system may be found, but, until that time, the user must select from existing types the one which in principle seems most applicable, not only to the character of electrical energy to be measured, but, also, to the conditions under which measurement must be made.

There is no one line connected with the electrical industry in which so much attention must be paid to details as in the manufacture of electrical indicating instruments. A very prominent engineer remarked to me recently that he had examined the construction and operation of a great many different types of indicating instruments, and, while a number of them bore evidence of similarity in design and construction, yet, in some cases, the manufacturer seemed to have acquired the "trick" of proportioning, manufacturing and calibrating, while others utterly lacked the "trick." As a matter of fact, it is not altogether a case of acquiring the proper "trick," but is a case of studying the theory and design of instruments for a number of years, backed by a large and varied experimental experience, which marks today the difference between the successful and unsuccessful instruments in the various types commercially exploited. Account must be taken of the most petty details of construction, the most careful and skilled labor must be employed in each department, and every particle of material entering into the instrument must be carefully selected and tested with reference to its particular function.

Personally, I have seen the experiment tried of taking a finished and efficient instrument and placing it in the hands of a careful and skilled mechanic for duplication; every wire and every part would be, apparently, an absolute copy of the original, and yet the results obtained would be totally different. It follows, therefore, that it is always wise in placing orders for electrical instruments to select a house who have publicly demonstrated the fact that they have learned the necessary "trick" or "tricks" of their profession.

In no line does reputation count for more than in the

manufacture of indicating instruments, nor is there any line in which I have been brought in contact where it takes so long a time to establish a reputation. Further than this, there is no line in which a greater amount of patience is required by the manufacturer. The average purchaser is totally ignorant of the laws or principles embodied in the instrument he is using, and, without regard to the fact that he may have selected the wrong type for the particular class of measurement he wishes to make, unhesitatingly blames the manufacturer for not having known what he wanted and for not having supplied him with the proper type of instrument by pure intuition.

Another obstacle which is commonly met by instrument manufacturers is the fact that for the average installation he is required to furnish instruments delivered to the switchboard contractor, who may or may not handle the instrument properly. It is true that most instruments are sealed, yet it is perfectly possible to utterly ruin the calibration of almost any type of instrument by improper handling without destroying the seal or opening the case. For instance, if a D'Arsonval instrument is laid upon the frame of an actively excited dynamo or motor, its entire calibration will be almost instantly changed, and yet no external evidence is presented to show exactly what has happened. Rough handling and an occasional fall, or improper packing for delivery to the plant, may further operate to destroy the accuracy of the calibrated instrument; yet the manufacturer is expected to provide instruments which will stand such usage and which, after they have been installed, may be subject to the influence of such strong external fields or such high external temperature as to make their correct indications impossible; and, after all of this, must have his instruments accepted or rejected by the consulting or supervising engineer, who, on the day of the test, carefully checks them with his personal portable instruments, which are usually kept in almost ideal condition and seldom or never leave his immediate possession. Some day I trust these conditions may be modified to the extent that the engineer in charge will test and check the instruments, if

he doubts the accuracy of the maker's calibration, before the instruments are shipped; or else issue instructions that the switchboard shall be drilled from template and the instruments set in place by the engineer's own assistants. In several cases I have succeeded in having the latter course adopted, with the result of mutual satisfaction on part of the owner, engineer and ourselves.

If arrangements could be made to have all instruments installed under the supervision of the engineer in charge, it would then be quite sufficient to have the specifications state the limit of error allowed in the indications of the voltmeters and ammeters; and this information, coupled with a specification covering the style of case, character of scale, range and class of measurement to be made, would enable the manufacturer to make an intelligent tender for the instruments required in any installation.

While a high degree of accuracy is desirable in all indicating instruments, it is particularly important that the voltmeter should be right within at least 1 per cent., since a variation of 1 per cent. in voltage means a corresponding variation in the candle-power of every incandescent lamp which may be in circuit. It is quite well known that the candle-power of any incandescent lamp varies directly with the voltage, the variation being approximately 1 candle-power for every volt increase or decrease from the normal voltage of the system.

Further than this, a variation of 1 per cent. from normal voltage on a system carrying incandescent lamps means a variation of about 16 per cent. in the life factor of the lamp and about 3 per cent. in the watt consumption per candle-power. Errors in the indications of ammeters do not produce such serious results, yet it is desirable that they should be accurate in order to obviate the danger of overload on circuit, translating device, or generator; and in the case of test instruments, as high a degree of accuracy should be demanded as is required in the voltmeter.

While on the subject of accuracy of indications, I would like to impress upon you the fact that, while we have three electrical units in common use, namely, the volt, ohm and

ampère, the ohm and ampère are the only two fundamental units, the volt being a derived unit. The absolute ohm may be quite readily obtained, and, as you probably know, is defined by law as the resistance offered to an unvarying electric current by a column of mercury at the temperature of melting ice, '14·4521 grams in mass, of a constant cross-sectional area, and of the length of 106·3 centimeters. The ampère is likewise defined by law as the practical equivalent of the unvarying current, which, when passed through a solution of nitrate of silver in water in accordance with standard specifications, deposits silver at the rate of ·000118 gram per second. The volt is defined as the electro-motive force necessary to send 1 ampère through 1 ohm. This, as you will note, gives us fundamental values for the ohm and ampère and defines the volt as a derived unit, and I am laying stress on the matter for the reason that the average electrical writer defines the volt as a fundamental unit equivalent to $\frac{1000}{1434}$ of the potential at the terminals of a standard Clark cell, and then defines the ampère as that current which 1 volt will cause to flow through a resistance of 1 ohm. As a matter of fact, the Clark cell is a secondary standard, exceedingly useful in practice, but not designed for use as a fundamental standard, due to the fact that Clark cells will vary among themselves and will give a gradually decreasing voltage at their terminals after they have been in service any length of time. It is, further, impossible to take 1 ampère from a single Clark cell without permanently ruining it; and, as a matter of fact, the potential of a Clark cell is only correct when the cell is used on open circuit.

It has been rather difficult for me to understand why the absolute ampère has been so little used or so seldom referred to, when it is a unit more readily verified than is possible with the volt. It is true that the fundamental method of determining the ampère is rather slow and tedious, but it is extremely easy to obtain the absolute ampère in c. g. s. units by the use of the tangent galvanometer. Defined in c. g. s. units, the ampère is such a current that, passed through a conducting wire bent into a circle of the

radius of 1 centimeter, will attract a unit magnetic pole held at its center with a force of one dyne; and this method applied to the tangent galvanometer gives the very simple formula of current equals galvanometer constant into the tangent of the angle of deflection of the polarized needle, the galvanometer constant being readily determined from its dimensions and the number of turns of wire, in connection with a determination of the value of the horizontal component of the earth's magnetism. Having once determined the absolute c. g. s. value of the ampère, we know that the practical ampère is $\frac{1}{10}$ the value of the c. g. s. unit. Having obtained the absolute standards of current, expressed in ampères, and resistance, expressed in ohms, the standard volt is readily determined from the relations given in Ohm's law. With the primary units once determined, all others, such as the farad or coulomb, can be readily obtained.

To obtain absolutely correct standards of voltage and current for alternating current calibration, it is only necessary to calibrate a dynamometer voltmeter and a dynamometer ammeter in true volts and ampères, as determined by fundamental standards; then, as a dynamometer measures directly the square root of the mean square, it will indicate correctly the virtual voltage or current in an alternating circuit. In calculating resistance in alternating current instruments, it is necessary to modify Ohm's law to the extent of substituting impedance for resistance. Since impedance is the vector sum of the ohmic and inductive resistances, it is readily obtained by triangulation when the two resistances are known, or when one is known and the angle of lag can be ascertained.

While the design, workmanship and material entering into the construction of an instrument are of vital and fundamental importance, the actual calibration is equally important. Assuming the manufacturer has reliable standards of voltage, current and resistance, trained observers are necessary who should be instructed not only to properly mark the scale, but to check it; and should also be instructed to carefully test the instrument for errors due to

reversal, to lag or hysteresis, and for errors introduced by the presence of external fields, or by the variations of internal or external temperature. Certain definite limits must be set for errors allowed, and no instrument permitted to go out until it has fully come up to the standard so set. In the company with which I am associated, we not only make all of these tests and check readings, but, likewise, provide an additional check on the calibration by plotting the calibration curve at the time the scale is made by the draughtsman, and if errors or irregularities are observed in the curve showing a departure from the standard curve applicable to the type of instrument under construction, the instrument is promptly returned to the calibrating room to have its readings checked, or to hunt for and remove the defect which caused the irregularity. It is a known fact that for every type of system there is a characteristic scale, following a more or less complex law, and any variation of the calibration from the normal indicates immediately that there is something wrong, either in the construction of the instrument or of some of the elements entering into it; or else it was inaccurately calibrated. I, personally, attribute a great deal of our commercial success to this intermediate checking in the drawing-room, and you can put it down as a settled fact that any manufacturer who produces instruments of any particular type the scales of which for corresponding ranges vary greatly from one another has not yet mastered the proper design or construction of his instrument.

It would be impossible to detail the many different kinds of measurements which can be made with the proper type of voltmeter, ammeter and wattmeter. In general, however, the possession of these three instruments enables one to measure not only voltage, current and power, but, likewise, capacity, inductance and resistance; and from these various measurements may be obtained data covering the performances of almost any type of generating or translating device.

There are many special forms of indicating instruments designed to indicate directly some of the various measure-

ments most frequently employed, two of which, at least, are so universally used at the present time that they deserve, at least, passing mention. One is a voltmeter designed for use on constant current arc light circuits, capable of indicating directly the total electro-motive force of the dynamo, and so connected by means of self-contained switches that not only can the total voltage of the dynamo or circuit be read, but, likewise, the presence of a ground indicated and its actual value in volts directly determined. This involves an arrangement of connections so that the voltmeter can be connected directly across the terminals of the circuit and then connected successively from the plus and minus side of line to ground. From the three readings thus obtained the number of lamps burning can be directly ascertained, the presence of the ground shown and the ground itself can be absolutely located by a very simple calculation. By employing, in addition to this, the known resistance of the instrument, the actual resistance of the ground in ohms may be obtained, thus defining its character.

An instrument of this character was brought out by my company some few years ago, and has met with almost universal favor, due largely to the growing tendency to measure resistances under full working potential. Such an instrument will show grounds which would not be shown by the ordinary galvanometer and bridge method of testing, and further enables the switchboard attendant to locate grounds while the line is in operative condition.

Another instrument which, for a time, fell into disuse, but which to-day is being used more extensively than ever, is the differential voltmeter, designed primarily to show the difference in potential between the bus bars and any dynamo which has to be connected to the bus bars in parallel with dynamos already operating. It is rather a curious fact that the average dynamo tender places implicit confidence in the indications of his differential voltmeter. The mere fact that he can see the pointer come back to zero when the free dynamo is being brought up to voltage, and the knowledge that when the pointer does stand at zero he

can throw the free dynamo into circuit without danger of trouble, impresses him with the idea that the instrument is essentially reliable and accurate, and he is ready to condemn any or all of his regular switchboard voltmeters which fail to agree with it in indication. This tendency has led my company to devote special attention to the calibration of differential voltmeters and to furnish them with a full scale which indicates the exact voltage of the bus bars, in place of furnishing instruments which give full scale deflections for 10 per cent. or 20 per cent. of the normal voltage.

In closing, I would say that I know of no subject connected with electrical engineering which promises such rich rewards for investigation as the subject of electrical measuring instruments, and if my hasty and very general review of the subject will influence any one to pursue the subject thoroughly and scientifically, I shall feel more than repaid. I have confined practically my entire time and attention to this line for a number of years and find the subject grows more interesting the further I investigate it, and I hope the time is not far distant when the literature on this subject will be as complete and as comprehensive as that now available in allied branches of electrical engineering.

THE FRANKLIN INSTITUTE.

Stated Meeting, April 25, 1899.

ELECTROMAGNETIC MECHANISM, WITH SPECIAL REFERENCE TO TELEGRAPHIC WORK.

By R. A. Fessenden.

(*Continued from vol. cxlix, p. 470.*)

Any motion which reduces the energy must have a force. Thus, since the energy would be reduced by enlarging the size of the plate of the condenser, keeping the charge constant, the plates of a condenser tend to stretch themselves out sideways if they are prevented from coming together. Thus we can make a nice little electrometer by taking a

piece of microscope glass, or a bit of a thin glass bubble, silvering one side of it and putting a drop of mercury on the other side, the glass being horizontal, with the silvered side down. A drop of mercurous chloride may be dropped on the mercury to take away its surface tension if it is desired to have great sensibility. Then if we put a refractometer over the mercury drop and connect one terminal of the cell whose voltage it is desired to measure, to the silver and the other to the mercury, we shall see the interference fringes move and can measure the voltage. If still greater sensibility be desired, we may put the unknown voltage in series with a known voltage of much higher value.

We may next consider the dielectric circuit *Fig. 1*, in

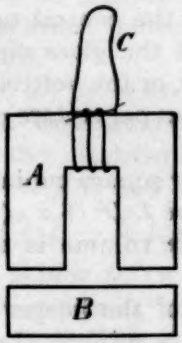

FIG. 1.

which we have a circuit consisting of the glass horseshoe A, and keeper B, these being in contact.

By winding turns of iron wire, C, around the glass horseshoe, and making the magnetic flux in the iron wire vary, we get a voltage round the glass circuit numerically equal to the time rate of change of magnetic lines passing through the central opening in the glass circuit. This voltage divides itself up inversely proportionally to the capacity ($\varkappa / 4\pi \times$ area $\div$ length = capacity) of the different parts of the circuit. If both keeper and horseshoe are of the same cross-section, the voltivity, F, will be equal to the total voltage, E, divided by the total length of the circuit. The total flux of electrostatic lines will be numerically equal to $E \times (\varkappa / 4\pi \times$ cross-section / length$) = Q$, or, $D = F K$.

In this case the electrostatic lines flow around the glass circuit, just as magnetic lines round an iron circuit. There is more leakage, however, in the case of the glass circuit than generally in the case of the iron, as $\varkappa$ for glass is only about three times what it is for air, and μ for soft iron may be 3,000 times μ for air. For highly saturated iron, however, the value of μ may fall to 2 or 3 and the distribution of the magnetic lines become exactly the same as that of the electrostatic lines.

Let us calculate the pull with which the glass keeper is attracted to the glass horseshoe. We will first calculate the energy in the circuit when the keeper is on.

If the circuit be of length L, area 1 square centimeter cross-section, capacity $\varkappa$, and the time rate of change of the magnetic flux through the central opening is $d P / d t = E$, the total voltage round the glass circuit, then the drop of voltage per centimeter, or the voltivity F, equals E / L. The flux of electrostatic lines per square centimeter will be then $F \varkappa = D$.

Since the energy per square cubic centimeter is $D^2 / 8 \pi \varkappa$, the total energy will be $L D^2 / 8 \pi \varkappa$ (the cross-section being 1 square centimeter, the volume is equal to the length $\times$ unity).

Next pull one side of the keeper away from the horseshoe so that there is a minute crack there, of length l. Then the energy will pile up in that crack and will flow away from the rest of the circuit, also the total amount of energy in the circuit will be lessened.

Now this is a case where, the voltage being kept constant, the dielectric is changed in configuration, and as we saw before, this means that the exciting circuit has a change of energy equal to double the change of energy stored up in the dielectric circuit, *i. e.*, equal to the sum of this change and of the mechanical work done. In this case the work done is negative, *i. e.*, work is spent in pulling the keeper, and consequently the energy in the dielectric circuit decreases by the same amount, whilst the change of flux in the dielectric circuit makes an increased gilbertance in the magnetic circuit linked with the horseshoe, and the mag-

netic flux jumps up as we pull the keeper away and tries to keep the electrostatic flux constant. Consequently both these quantities of energy go to the magnetic circuit, and since we have no true magnetic current (there may be such a thing, but the method of producing it has not yet been published), ultimately appears in the electric circuit which causes the change of flux in the magnetic circuit threading the horseshoe.

To calculate the amount of energy now resident in the circuit we see, since the electrostatic lines have been reduced owing to the increased reluctance of the circuit on account of the gap, from D to D_1, the energy in the glass is now $L\,D_1^2 / 8\,\pi\,\varkappa$ and that in the gap is $l\,D_1^2 / 8\,\pi\,\varkappa_1$ (since the same lines thread the air gap as thread the glass and the cross-sections are the same, the value of D_1 is the same for both air gap and glass).

We find the value of D_1 by considering that the total voltage, subtracted from that used up in the gap is the amount left for driving the electrostatic flux through the glass. So the new value of F is now $E - D_1\,l / L$ and

$$D_1 = \varkappa\,(E - D_1\,l / L)$$

The energy in the glass is now $D_1^2 / 8\,\pi\,\varkappa$ times the volume of glass and that in the gap is $D_1^2 / 8\,\pi\,\varkappa_1$. Adding these and subtracting the sum from $D^2 / 8\,\pi\,\varkappa$ (the original energy in the glass), we get, on simplifying the result:

Total change of energy is

$$D\,D_1 / 8\,\pi\,\varkappa_1$$

But when the crack is indefinitely small, D_1 varies infinitely little from D, and the formula becomes

$$D^2 / 8\,\pi\,\varkappa_1$$

When the substance in the gap is air, for which $\varkappa_1$ is unity, the formula becomes

$$D_1^2 / 8\,\pi$$

So much for the dielectric circuit. I have gone into it in some detail so as to show the general method of working and to obtain a certain depolarization of ideas in regard to

fluxes. To apply our equations to the magnetic circuit all we need to do is to change D to B, $\varkappa$ to μ, and F to H.

Take the case of a permeameter. Here instead of a glass horseshoe we have an iron one, instead of an iron wire carrying a varying magnetic flux we have a glass wire carrying a varying electrostatic flux, or more simply, a copper wire carrying a current. Just as the voltance in the glass circuit was given by the time rate of change of magnetic induction, so the gilbertance in the iron circuit is given by the time rate of change of electrostatic flux, or since unit quantity of electricity is, for our sins, defined as being the origin of 4π electrostatic lines, by $d / d t (4 \pi Q) = 4 \pi \times$ current, and the force required to pull the core in two is given by

$$B^2 / 8 \pi \mu_1$$

per square centimeter of cross-section of the core.

It will be noted that this formula is different from that given in books on the subject. This is because the formula generally given, *i. e.*, $(B^2 - H^2) / 8 \pi$, when the coil is not moved with the core, is incorrect. For it makes absolutely no difference as regards the force whether the coil is moved or remains at rest with respect to the pulled piece. It is unnecessary to enter into a discussion of the way in which this error arose, but we may easily show that the H^2 should not be in the formula.

For, consider the magnetic circuit when the core has been pulled apart a small distance, the coil remaining stationary. Fasten the coil to the moved part of the core and replace that part in its first position. If the usual formula be correct, more energy will be gained on the backward movement than was expended on the forward one. Now release the coil from the core and move it to its first position. Both theory and experiment agree as to the result, that no work will be done in making this last movement. Consequently, by repeating the operation we have a source of unlimited energy, or a perpetual-motion machine in the ordinary usage of that term.

The formula

$$\text{Average force} = B B^1 / 8 \pi \mu_1$$

when a portion of the circuit is moved to such a point that the original flux changes from B to B^1, the medium in the gap, being of permeability μ_1 will be found to be very generally useful in practice, where the amount of magnetic leakage does not vary during the motion to an appreciable extent compared with the amount of the main flux B.

Before leaving the subject it may be well to point out that pulling tests run a very great chance of being inaccurate through the change of induction as the crack begins to form. When one side lifts first the lines will move to the other side, thus increasing the value of B there, and as the total P remains the same as before, the average B^2 is much larger. To a certain extent this occurs of course and that it does not give quite large errors is possibly due to the fact that polished metal takes a skin, and that, as Ewing has shown, the two cut surfaces, no matter how well polished, always have a reluctance equivalent to an air gap of about ·003 centimeter in length. The presence of this initial virtual air gap renders the effect of slight tilting very much less than it would otherwise be; so also may the effect of self-induction, for this would oppose any sudden change in the position of the lines. The self-induction of the magnetizing coil also tends to keep the total amount of number of lines constant.

On the whole, however, the permeameter gives quite sufficiently exact results for most practical purposes, and all these different effects, as well as that due to the change of flux with change of mechanical stress on the core, seem to average up somewheres near the correct result. The following is a simple and correct formula for use with the permeameter:

$$B = \frac{4\sqrt{2F}}{d}$$

where d is the diameter of a circular rod and F the force in dynes.

For accurate work, however, I prefer the following device, *Fig. 2*, constructed three or four years ago, by Mr. Willyoung, to my designs, which enables induction and hysteresis

tests to be made of both bar and sheet iron with very considerable accuracy.

The bar, 1 centimeter in diameter, magnetized by the coil *K*, has a screw thread cut on each end, by a very correct die, and is screwed without twist in the blocks *A C*. There is thus very good magnetic contact all round and the actual reluctance is easily measured by turning the rod to different diameters or by using rods of two lengths. This latter method was first published by Ewing, some years ago, but this apparatus was then a year old.

A grid of wire *H*, hung in the slot *F*, has a current passed through it till it moves a certain amount, determined by

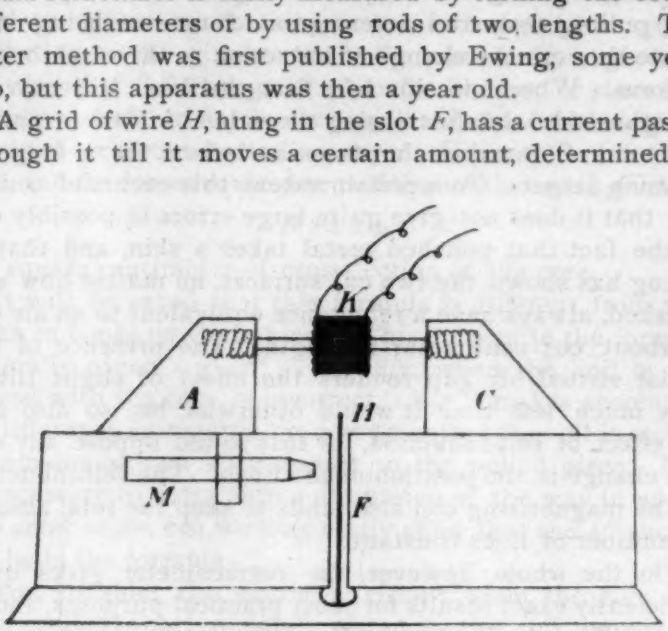

FIG. 2.

the microscope *M*, and the amount of current necessary to do this is inversely proportional to the flux. It works very well when the grid is properly wound, but for some reasons it is more convenient to wind a few wires on a frame and drop them through the slot, observing the effect on a ballistic galvanometer.

Sheets of iron are cut to size and their ends interleaved in a pair of pieces similar in size to those used for the solid bars. One advantage of this method is that if any error is taken in a reading it can be repeated, and, in fact, it seems remarkably well adapted to the work.

A peculiar case is that shown in *Fig. 3*, where one pole piece is of greater area than the other, one being, say, 1 square centimeter and the other 4 square centimeters. According to theory, the pressure on the one face should be, if the total number of lines is P, $P^2/8\pi\mu$ and that on the other, $4(P/4)^2$, one being thus four times as great as the other. For a time I felt rather disgusted with the perpetual-motion men, that they had not hit upon the simple device of fixing such a magnet on an axis passing through its plane and driving unlimited machinery with it, the acknowledged and patent presence of the magnet rendering the detection of extraneous electric power rather difficult, until I remembered Stevenson's fisherman—"It is in my

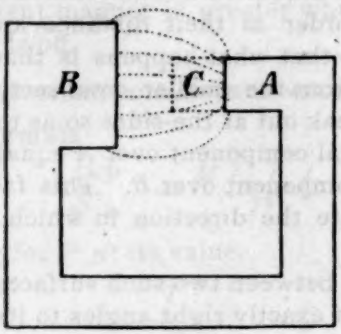

FIG. 3.

thought that one thing is as good as another," and reflected that perhaps they were wiser in their generation and were more concerned about finding the dupes than the means for duping.

The explanation, of course, is that when you move the pole A up to a short distance towards the pole B, you do not cut out a slice of magnetically stressed space C, but really change the whole configuration. Now, it will be seen that, in addition to changing the flux in space C from that which was originally in the air and ether to that which is now in the ether and iron, we have also moved together all the lines. Thus work has been done against their mutual repulsion. So that the real force is got by taking the energy

gained and dividing by the distance. It is evident that the change of configuration will be the same whether it is A or B that is moved, provided they are moved the same distance, and, the change of work being measured by the initial and final states being thus the same, for a given movement, the force will be the same.

How about the $B^2 / 8\pi$ forces then? Well, I have not calculated out the direction of the lines for any particular case, in fact it is practically impossible to do so, for though the methods of conjugate functions which have been applied by Maxwell, Kirchoff and others give us the solution of some simple case in two dimensions, yet they are not applicable to such cases as this, where the permeability changes with the induction and the dimensions of the faces are of the same order as their distance apart.* But it is fairly easy to see that what happens is that the lines bow themselves out from the smaller cross-section, as shown in the figure, and leak out at the sides so as to make the integral of the normal component over A equal to the integral of the normal component over B. This fact can be made use of to calculate the direction in which the lines leave the face.

Thus the pull between two such surfaces, since the lines cannot leave B at exactly right angles to its surface owing to their mutual repulsion, must be very slightly less than the value of $B^2 / 8\pi$ at the surface of B, multiplied by the area of B.

A simple way of seeing this is to let both poles be the same size at first. Separate them a certain distance and then let one contract in cross-section. The work done in contracting gives a force at right angles to the line of joining the faces, and so does not affect the pull. Similarly, when, as in an air gap, the flux passes from a substance of one permeability to one of a different permeability, the purely magnetic forces on the two sides cannot be equal. It is

*The method which has been suggested, and even employed, of drawing straight lines from one surface to another, deducing the flux from them, leads, of course, to absurd results.

usual in electrostatics to attribute such effects to fictive layers of electricity, in the attempt to make the electrical forces balance each other across an interface. This artifice is without physical justification, as Heavyside has shown ("Electrical Papers," Vol. I, p. 247). My own theory (hypothesis) is that the balancing is done by purely mechanical forces, *i. e.*, that in a permanent magnet, for example, the ends of the air gap, when the keeper is off, pull the two ends of the magnet together, the molecular force balancing the difference in the magnetic forces. Generally speaking, it is surprising how little we know about the workings of a permanent magnet, and the subject is well worthy of investigation. Some things, however, we can see at once. For example, that the flux in a permanent magnet is greater when the keeper is on than when it is off.

TYPES OF ELECTROMAGNETIC MECHANISMS.

Taking the formula

$$\frac{Q\,P}{x^{\frac{1}{2}}\,\mu^{\frac{1}{2}}} = \frac{M\,L_x\,L_y\,L_z}{T^2}$$

and substituting for $x^{\frac{1}{2}}\,\mu^{\frac{1}{2}}$ its value,

$$\frac{T}{L_z}$$

we get

$$\frac{Q\,P}{T} = \frac{M\,L_x\,L_y}{T^2}$$

The energy stored up in the magnetic field due to the strain is given in various forms, for instance, as $\frac{1}{2}\,L\,I^2$, where L is the coëfficient of self-induction, and as

$$\frac{H\,B}{8\pi} \times \text{volume}$$

These are equivalent, as may be seen by taking the case of a closed ring.

For $L\,I$ is the total number of linkages
= lines × turns = B × cross-section × turns.
since B × area = total number of lines.)

$$I = \frac{\text{gilbertance}}{4\pi \times \text{turns}} = \frac{H}{4\pi} \times \frac{\text{length}}{\text{turns}}$$

since the total gilbertance divided by the length of the magnetic circuit equals H.

$$\frac{1}{2} \mathrm{L} I^2 = \frac{B}{2} \times \text{cross-sect.} \times \text{turns} \times \frac{H \times \text{length}}{4\pi \times \text{turns}}$$

$$= \frac{H B}{8\pi}$$

In the case of a very long solenoid, also, the same is easily proven if we remember that the gilbertivity outside the solenoid is negligibly small.

We may pass a current about a magnetic circuit so as to obtain one of three results:

(1) Expend electrical energy equal in amount to $\frac{1}{2}$ L I^2, this being stored up in the dielectric and lost when the circuit is broken, being converted into heat.

(2) Expend electrical energy equal in amount to L I^2, one-half of this, $\frac{1}{2}$ L I^2, being stored up and lost as before, the remainder, $\frac{1}{2}$ L I^2, appearing in the form of mechanical energy which can be utilized.

(3) Expend electrical energy equal in amount to 2 L I^2, all of which appears as mechanical energy and can be utilized.

The efficiency of the first method is zero, of the second, 50 per cent., and of the last, 100 per cent.

Practically all electromagnetic mechanisms, with the exception of electric motors, belong to a combination of the first and second classes.

Case I. — In Case I we complete the magnetic circuit first, and then produce a magnetic flux in it by passing a current around it. The linkages at any moment being L I, the back voltage, which is numerically equal to the time rate of change of linkages, is

$$\frac{d}{dt} \mathrm{L} I$$

and the power

$$\frac{dW}{dt}$$

is at any instant

$$I \frac{d}{dt}\left(\mathrm{L}\, I\right)$$

Therefore,

$$W = \frac{1}{2}\mathrm{L}\, I^2$$

This is the energy needed to push the electricity along the wire against the back voltage. As nothing material moves, we get no mechanical work (if we neglect the slight expansion or contraction of the iron itself). When the current is stopped, the energy returns to the circuit; or, if the circuit is broken, it appears in the form of heat, in the spark or in the wires, due to oscillatory currents set up in them. I have said that this energy may come back to the circuit. This is not strictly true, for magnetic storage of energy is very inefficient. It is for this reason, as I have shown,* that single-phase currents cannot be efficiently split into two-phase by using capacity and self-induction. The energy stored in a closed magnetic circuit, as mentioned before, is

$$\frac{H\, B}{8\, \pi}$$

per cubic centimeter. But, as we shall see later,

$$H = \frac{a\, \beta}{(1 - \beta\, B)}$$

where a and β are constants. So the energy per cubic centimeter is

$$\frac{a\, B^2}{8\, \pi\, (1 - \beta\, B)}$$

The loss in hysteresis, per cycle, is by Steinmitz's law, for a fair brand of iron, for which $a = .0003$ and $\beta = .00006$.

$$\eta = .003$$

The efficiency is

$$\left\{ \frac{a\, B^2}{8\, \pi\, (1 - \beta\, B)} - \eta\, B^{1.6} \right\} \div \frac{a\, B^2}{8\, \pi\, (1 - \beta\, B)}$$

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If $B = 10{,}000$, this reduces to

$$\frac{185}{375}$$

or not quite 50 per cent.

It is for this reason, as I have shown elsewhere,* that the presence of iron in a transformer cannot distort the curve of voltage, and that the curves of impressed back and secondary voltage cannot differ from each other by more than 1 per cent., and that the current curve also cannot be distorted except at loads of less than $\frac{1}{10}$ of full load, as otherwise, since distortion in such a case would mean a storing up of energy during a part of the period and the giving of it out again later, if distortion occurred to an extent greater than 1 per cent., the transformer would be so inefficient that it could not be sold. Hence the very common jibes about ironless transformer mathematics are senseless, and can only be made by those who have had no practical experience in electrical design.

Case II.—In this case the magnetic circuit is at first open, and supposed of infinite reluctance. The electric circuit being closed and the current flowing, the magnetic circuit tends to close itself, for the potential of a coil carrying a current is given by the formula

$$P\,n\,I$$

where P is the total magnetic flux, I the current in the coil and n the turns. This potential is negative when the lines are passing through the coil in the direction in which the current in the coil would tend to make them itself, and so, as the potential has its lowest (its greatest negative) value when the coil is enclosing as many as possible of the lines it produces, the circuit tends to move in such a way as to enable more lines to be made. At the end of the motion, when the armature has come to rest, the energy in the dimagnetic is as before,

$$\frac{1}{2}\,\mathrm{L}\,I^2.$$

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* *Ibid.*

But, as the armature moved, more and more lines were formed inside the coil, and these made a back voltage tending to choke back the current. The power which the electric circuit must furnish to keep on driving the electricity through the coil against the back voltage is calculated as follows:

The back voltage is, as before, the time rate of cutting of linkages, *i. e.*,

$$\frac{d}{dt} \mathrm{L}\, I.$$

But it is now L which is the variable, for the current was established before there were practically any lines threading the circuit. The power is, as before,

$$\frac{dW}{dt} = I \frac{d}{dt} \mathrm{L}\, I, \text{ whence}$$
$$dW = I^2\, dt \text{ and}$$
$$W = \mathrm{L}\, I^2$$

Half of this is spent in storing up the energy

$$\frac{1}{2} \mathrm{L}\, I^2,$$

which is equal in this case (since $Pn = \mathrm{L}\, I$, all of P being due to the current in the circuit we are considering; if it were not, Pn would not equal $\mathrm{L}\, I$, as P is the *total* flux, no matter how derived, whilst $\mathrm{L}\, I$ is the flux due to the current I) to

$$\frac{1}{2} Pn\, I,$$

in the magnetic circuit. This is wasted when the current is broken, appearing as heat. The rest,

$$\frac{1}{2} \mathrm{L}\, I^2 \text{ or } \frac{1}{2} Pn\, I,$$

is available for mechanical work, and if not so spent, turns to heat, through the armature hitting against a stop, or, by oscillating back and forth, wastes this energy in heat through friction.

This method of getting work from electric energy is that usually used in electromagnetic devices. Though it may, theoretically, have an efficiency of as nearly 50 per cent., yet, as will be seen later, it is generally arranged so as to give but 5 per cent. to 20 per cent.

Case III.—In this method the magnetic circuit is not only closed, but has the flux passing through it before the actuating current is passed through the moving coil. The magnetic circuit may consist of either a permanent magnet or an electromagnet and an armature, both being fixed in position, and there being at least one air gap.

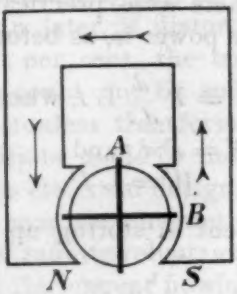

FIG. 4.

Fig. 4 shows such a device, where the coil *A* can rotate about an axis perpendicular to the plane of the paper. Taking the permanent flux *P* as passing in the direction of the arrow, from *S* to *N* in the iron, then if the coil be in position *A*, and has a current *I* in it, passing from back to front in the top of the coil, then the potential of the coil is $P n I$. It will then tend to move till it is horizontal to the position *B*, when it will enclose no lines and its potential will be zero. But by moving through a further angle of

$$\frac{\pi}{2}$$

or 90°, it can enclose *P* lines in the plus direction, its potential therefore becoming $-P n I$. It accordingly assumes this position and does $P n I$ more work, or $2 P n I$ in all.

This work is done by the electric circuit, for the wire, in moving, has a voltage generated in it at any time equal

$$2\frac{d}{dt}Pn$$

and this, multiplied by the current I, which we maintain constant, gives us

$$2\,I\,n\,dP,$$

whence, in one-half revolution

$$W = 2\,I\,P\,n.$$

This all appears as mechanical work, for if the number of ampère turns on the coil is small compared with the gilbertance which drives the flux P through the circuit, P is practically constant. Suppose, however, that the current in the coil produces a flux P', then at first the potential is

$$(P - P')\,n\,I$$

when the current is made, and

$$\frac{1}{2}P'\,n\,I$$

work is done in making it. This work is furnished by the electric circuit A. During the first 90° of rotation work

$$(P - \tfrac{1}{2}P')\,P\,I\,n$$

is done approximately, and during the next quadrant,

$$(P + \tfrac{1}{2}P')\,I\,n,$$

the total being $2\,P\,I\,n$. The energy

$$\frac{1}{2}P'n\,I$$

which was originally spent in starting the field is lost, appearing as heat when the circuit A is broken, and if P' is small compared with P, then, as mentioned above, the loss is negligible and the efficiency is practically 100 per cent.

These three cases may be illustrated by a simple mechanical analogue. Take a spring with a pan at its lower end as

in *Fig. 5*. Pour gradually 10 grams of sand into the pan, so that the lip of the vessel from which the sand is poured is always just touching the pan. The pan will, in descending, stretch the spring to a length of say 20 centimeters, and as the average weight in the pan was 5 grams, the total work done is 100 gram centimeters. Now tilt the pan and let the sand fall out, when the energy will be spent as heat, through the oscillatory motion of the spring. We have thus spent 100 grams of work and got nothing for it. This is the analogue to the first case.

Next add the 10 grams all at once. The pan will immediately descend with considerable velocity, and if we fix a wheel and pulley to it, we can get 100 grams mechanical work, as 10 grams moving through 20 centimeters

FIG. 5.

gives 200 gram centimeters of work, and there is only 100 stored up in the spring. If we now tilt out the sand, we only lose 100 gram centimeters, and our efficiency is 50 per cent. This is the analogue of the second case.

Now gradually pour, as in the first case, 10 grams into the pan. The spring is extended and comes to rest. This represents the formation of the permanent magnetic field in the third case. Then take 1 gram and drop it from the original position of the pan. When it reaches the pan it will have done 10 units of work, all of which we can get out in the shape of mechanical work, by letting it come down slowly. After it has reached the pan, the spring lengthens 2 centimeters more, doing 22 centimeter grams of work, of which, when the 1 gram is tilted out at the bottom, 2 gram centimeters is wasted. We have thus done 22

centimeter grams of work and utilized 20 of them. The efficiency is thus nearly 100 per cent. This is the analogue to the third case.

In each of the above cases, the circuit has been supposed to be broken when the movement was ended. What would happen if, instead of breaking the circuit, the voltage, and hence the current, were made periodic, and alternating?

In Case I, the energy would, to the extent of 50 or 75 per cent., come back to the circuit and be saved.

In Case II, the energy would come back to the electric circuit, but would not be available, except a fraction of it, as it would come back as a lot of higher harmonies of no particular use, and would fritter itself away in heating up the wire.

In Case III, after the current has once been started, *i. e.*, after several periods, the energy may be made to come back entirely, so that none of it is wasted. To accomplish this, however, the wire must move synchronously with the generator and the shape of its curve of back voltage must be identical in form with the voltage curve of the generator.

It will be noted that we have altogether neglected to take into account the $I^2 R$ loss of the magnetizing coil. This will be dealt with later, as it is entirely independent of the

$\frac{1}{2}$ L I^2 loss.

MOST EFFICIENT DESIGN.

As Case I is only of importance in certain instances which will be taken up later, we will deal with mechanisms of Class II first.

In these the current is flowing before the magnetic circuit is closed. As mentioned before, the energy obtained mechanically is only half that expended during the closing of the magnetic circuit.

But in most cases still more energy is wasted in that L is not zero at the instant that the armature commences its movement, and the mechanism is thus a combination of Classes I and II. Consider, for instance, a core in a solenoid.

Before the current is made, the coil has already an appreciable self-inductivity, and when the current is flowing at its final value we may suppose that the coil of 200 turns and 1 ampère makes a flux of 20,000 lines in the circuit. The linkages then are 20,000 × 200, and the value of L is then (20,000 × 200) ÷ (1 / 10) (1 ampère being 1 / 10 absolute unit of current). The value of

$$\frac{1}{2} \mathrm{L} I^2$$

is then 400,000 ergs, and, as we have seen, this amount of energy is wasted entirely. Suppose, then, that the plunger moves, till at the end of its stroke there are now 40,000 lines threading the circuit. The value of the coëfficient of self-induction is now (400,000 × 200) ÷ 1 / 10, and there are now 400,000 more ergs stored up in the circuit. But this increase has been made by varying L, not I, and falls under Class II, so that an amount of energy equal to this must have been available for mechanical work, *i. e.*, 400,000 ergs. Thus, when we made the current we expended 400,000 ergs, and whilst the core was moving we expended 800,000 more, of which only half was available for mechanical work. Our efficiency is, therefore, 33⅓ per cent. instead of 50 per cent., as it would have been if the core had been out of the way entirely at first.

Our first rule, therefore, is as follows:

Rule I. In mechanisms in which the actuating current produces the magnetic flux, the magnetic flux before the armature has commenced its motion should be as nearly zero as possible.

[*To be continued.*]